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BENJAMIN SILLIMAN.

OUR honored associate, Professor BENJAMIN SILLIMAN, the founder of this Journal, whose name has appeared upon the title page of every number, from the first until the present, is with us no more. He died at his residence in New Haven, early Thursday morning, Nov. 24, 1864 (the day set apart for a national thanksgiving), having reached the age of 85 years.

It becomes our duty to place on record in these pages, as an inscription to the monument which he has himself erected, an outline of his career and a tribute to his memory. Few men enter life with such promise as he; fewer still sustain themselves so evenly, and die so widely lamented.

Instruction in natural science has been his great work; and in it he was emphatically a man of the times. Beginning when almost nothing was known in this country of the departments to which he was especially devoted, he lived to see them carried forward to a high degree of progress, and their importance everywhere acknowledged. His life, which was one of few marked incidents, was passed in his native State, in connection with Yale College, the institution that early selected him as one of its faculty. Two or three times he was invited to become the president of colleges elsewhere, but New Haven continued his chosen home. Twice he visited Europe, first in 1805–6, in order to qualify himself for his work in life by attendance upon lectures in London and Edinburgh, and by observation of foreign institutions of learning; and again, near

the close of his life, in 1851, when he was accompanied by his son, and made a more extended tour of observation and inquiry. Frequent journeys in his own country made him acquainted personally with the institutions and the men of every State, while his habits of prompt and friendly correspondence perpetuated the intimacies which he formed at home and abroad.

Without attempting a formal biography (which the late day of his decease renders impossible at this time), we propose to speak briefly of Professor Silliman's career as an officer of Yale College, and as a man of science, and then of his personal character and influence in the community.

The Silliman family has resided in Fairfield, Conn., since the early colonial days. Tradition says that Claudio Sillimandi, their earliest known ancestor, was driven, in 1517, from Lucca, Italy, to Switzerland, by religious persecution. The descendants resided in Berne, and afterward in Geneva, whence they emigrated through Holland to this country about the middle of the seventeenth century. A worthy pastor of the name, living with his family near Neuchatel, was visited by Prof. Silliman in 1851.

Ebenezer Silliman, the grandfather of Benjamin, graduated at Yale College in 1727, and Gold Selleck, the father, in 1752. The latter was a Brigadier General of militia in the Revolution, and was entrusted for a time with the defence of the Long Island coast. In 1775 he was married to Mary, the daughter of Rev. Joseph Fish of Stonington, and the widow of Rev. John Noyes. The two children of this marriage, Gold Selleck and Benjamin, became members of the same class in college, and have maintained through life an intimacy peculiarly fresh and cordial. The younger brother, Benjamin, was born in North Stratford, Conn. (now the town of Trumbull), August 8, 1779. The elder, who was born in 1777, is still living in Brooklyn, N. Y.[1]

Throughout his active life, Professor Silliman has been identified with Yale College. He entered the institution in 1792, graduated in 1796, became a Tutor in 1799, was appointed Professor of Chemistry and Natural History in 1804; and in 1853, having been relieved at his own request from further service as an instructor, he was designated, by the Corporation, Professor *emeritus*. Thus, during a period of nearly three-quarters of a century, his name has appeared as a student and

[1] Prof. Silliman was twice married: first, in 1809, to Harriet, daughter of the second Gov. Trumbull of Connecticut, the mother of his nine children; and again, in 1851, to Mrs. Sarah Webb, daughter of John McClellan. Five children survive him, one son and four daughters. All are married, the eldest daughter to J. B. Church, the second to Prof. O. P. Hubbard, the third to Prof. J. D. Dana, and the fourth to Rev. E. W. Gilman. His descendants include twenty-three grandchildren, besides five deceased, and two great-grandchildren.

teacher successively on the catalogues of the college. He was a pupil both of Dr. Stiles and Dr. Dwight, and the colleague of the latter during eighteen years. With President Day and Professor Kingsley he was associated for half a century and more in the government of the institution.

In the capacity of a college officer, he was preëminent as a teacher. The professor's chair, in the laboratory or the lecture-room, was the place above all others in which his enthusiasm, his sympathy with youthful aspirations, his varied acquisitions, his acquaintance with the world of Nature and of Art, and his graceful utterance, exerted their highest and most enduring influence. The minds which he aroused to the study of Nature have become investigators and teachers in every portion of the country, and all his pupils, whether devoted to science or to letters, will bear testimony to the interest which he awakened in these pursuits. They will never forget the admirable tact with which the manipulations of the laboratory were performed, or the brilliant experiments in chemistry which the lecturer seemed to enjoy as if, like the class, he had never witnessed them before. The course in chemistry, in early years, extended through one hundred and twenty lectures. In later days it was not so long, but was followed by a course in mineralogy and another in geology. Here, too, Prof. Silliman had the same magnetic influence on his students, sending them off on long walks about New Haven and at home to search for specimens, or to study the phenomena of geology. The third of these annual courses, that on geology, he gave with peculiar zest and eloquence. He delighted to depict the catastrophes of geological history and to clothe the world with the plants and animals of former days.

Professor Silliman was less concerned in the government of the students than some of his associates; but questions were continually arising in which his counsel was of weight. He was prompt in rebuking every form of youthful delinquency, yet was never harsh or inconsiderate. No student ever left his presence feeling wronged or indignant. He would much rather sacrifice a rule than injure an offender. If he seemed sometimes to be lenient, it was the leniency of a father, for his mind regarded the improvement of his scholars rather than the enforcement of routine and discipline. His paternal lectures to the Freshman class on morals and manners were admirable in their influence, and many a graduate of the college will acknowledge that his habits for life were affected by the judicious hints which he received from his kind and sympathising teacher.

Mr. Silliman's labors began with instruction; but they did not end there. His active and versatile disposition led him to become interested in and to help forward whatever would con-

tribute to the welfare of Yale College. When he went abroad, in 1805, to fit himself for the duties of his professorship, the purchase of books for the library was one of the duties with which he was especially charged. He was one of the library committee until his retirement. In his own departments, not only the Chemical Laboratory, but also the Cabinet of Minerals owed its existence to his energy. This collection is indeed so important, that something more than the mere mention of it seems due. About the time when Mr. Silliman was appointed a professor, the entire mineralogical and geological collection of Yale College was transported to Philadelphia in one small box, that the specimens might be named by Dr. Adam Seybert, then fresh from Werner's School at Freiberg, the only man in this country who could be regarded as a mineralogist scientifically trained. From this small beginning grew the present cabinet. In 1810, owing to personal regard for Prof. Silliman, Col. George Gibbs deposited with Yale College his valuable collection of minerals, and, after it had remained open to the public fifteen years, various friends of the college, chiefly through the instrumentality of Prof. Silliman, subscribed for its purchase the sum of $20,000. Other important accessions were also secured through his influence, not only from college graduates and other American gentlemen, but from various foreign collectors.

The Clark telescope is another of the donations to Yale College due to Prof. Silliman. This excellent glass, the best in the country at the time of its purchase, was the means of exciting among the students of the college unusual attention to astronomical pursuits for many years after its reception. The liberal donor, a farmer near New Haven, by this and other more important gifts, placed himself foremost among all the benefactors of the college up to that time, and Mr. Silliman was the medium through whom his benefactions were bestowed. The Trumbull Gallery of Paintings, a collection of priceless value, not only as works of art but also as illustrations of American history and biography, was secured to the college through the same enlightened instrumentality.[2] The Medical Institution of Yale College and the Sheffield School of Science, important branches of the University, were both greatly aided in their beginnings by the influential exertions put forth by Professor Silliman. He was one of the chief founders of the Alumni association of the college, and at their anniversaries and on other occasions, he was, as another has said, "the standing 'orator' of the college,

[2] It is an interesting fact that as early as 1842, Prof. Silliman, in his Alumni address, pointed out the need of another edifice for the Fine Arts, or an extension of the Trumbull Gallery, at no distant day. This want is about to be supplied by the liberality of an alumnus of the college. Prof. Silliman was prevented by illness, a week before his decease, from taking a public part in the exercises of laying the corner stone of the proposed structure.

the principal medium between those who dwelt in the academic shade and the great public." Not unfrequently he was the college solicitor, asking funds for the expansion of the institution, and never asking in vain.

Although his services as a college officer were great, Prof. Silliman's strongest claim to the gratitude of men of science rests upon the establishment, and the maintenance, often under very discouraging circumstances, of the "American Journal of Science." The history of this undertaking has already been given, in his own words, in the introduction to the 50th or Index volume of the First series of the Journal; and it is for others, rather than for us, to give an estimate of his editorial services. It is but just, however, to call attention to a few circumstances, which all will regard as creditable to its founder.

He had the sagacity to foresee, as long ago as 1818, the scope which such a magazine should take. The prospectus which he then wrote is applicable almost exactly to our pages to-day. Experience has established the wisdom of the course which he marked out.

He maintained the Journal, from the beginning, at his own pecuniary risk. Its publication has often been a serious financial burden, and in its most prosperous days has not yielded a fair return for editorial labor. But it has been continued, at this personal inconvenience, for the sake of American science, that the labors of our countrymen might be made known abroad, and the labors of Europeans understood in this country.

The Journal has never been used for the benefit of any party or individual, but solely for the advancement and diffusion of scientific truth. Its pages have been always open to free scientific discussion, with truth as the single end in view.

The original investigations of Prof. Silliman are not numerous. In the early part of his career he began with energy some important experiments and researches. He undertook a geological survey of Connecticut; he published a paper in conjunction with Prof. Kingsley on the famous Weston meteorite; he applied the newly invented blowpipe of his friend, Dr. Hare, to the fusion of a variety of bodies, which were before regarded as infusible; he demonstrated in the galvanic battery the transfer of particles of carbon from one charcoal point to the other; he made scientific examinations of various localities interesting in their geological or mineralogical aspects. But he was too much needed elsewhere to be allowed to remain a close student in the laboratory, or to engage with constancy as an explorer in the field of geological research. He has probably been a more useful man in the wider spheres of influence to which he was called than he could have been in a life devoted to scientific investigation.

During a considerable part of his life, he was one of the few men in the country who could hold a popular audience with a lecture on science. The public early knew of his capabilities, and for many years he yielded to invitations from various parts of the country to deliver lectures on Geology and Chemistry. In 1833 he gave his first popular course on Geology at New Haven, which was repeated in 1834 at Hartford and Lowell, and in 1835 at Boston and Salem. At Boston, the audience desiring to attend was so much larger than the largest hall would hold, that each lecture was given twice for the accommodation of the public. From 1840 to 1843 inclusive, he gave four successive courses of the "Lowell Lectures" in Boston. Besides various other engagements in the Northern and Eastern states, he went in 1847 by invitation to New Orleans, and on his way appeared before crowded audiences in other cities of the South; and five years after the resignation of his professorship in college, when he had passed his 75th year, he made the long journey to St. Louis, in obedience to a call for a course of lectures from the citizens of that place.

In lecturing, his language was simple—his flow of words easy, generous and appropriate—his style animated, abounding in life-like and well-adorned description, often eloquent, and sometimes varied with anecdote running occasionally into wide digressions. His manner was natural, and every feature spoke as well as his mouth; his noble countenance and commanding figure (he was nearly six feet in height, with a well-built frame) often called forth, as he entered the lecture hall, the involuntary applause of his audience.

In his popular courses he often lectured on the subject of "Geology and Genesis," and as he was widely known not only as a man of science, but as a sincere believer in the sacred Scriptures, he greatly aided in removing from the religious world the apprehension that science and religion were hostile in their teachings.

Mr. Silliman found great pleasure in helping forward other men of science. He rejoiced heartily in their progress; his house and his laboratory were always open to receive them, and if a friendly word or letter from him could advance their interests, he was ever ready to bestow it. He also felt a deep concern for the advancement of scientific investigations in every part of the country, and whenever, in halls of legislation, or before the public, the name of Benjamin Silliman would advance a useful project, it was not withheld. In more than one instance, the foreigner, or the exile, remembers his kindness with almost filial devotion.

Prof. Silliman's scientific publications, apart from his contributions to this journal, were chiefly text-books. He edited

Henry's Chemistry and Bakewell's Geology, for the use of his pupils, and also published a work on Chemistry, in two volumes.

His long labors for science brought him honors from all parts of the world. His name is on the roll of several of the principal scientific Academies or Societies of Europe, and of those of his own country. He was one of the original members of the National Academy of Sciences, and a Regent of the Smithsonian Institution.

Aside from Professor Silliman's influence as an officer of Yale College, and as a well known man of science, his personal hold upon the community at large was remarkably strong. This was due somewhat to the favor with which his popular lectures were received, and to the wide circuit over which he had journeyed. It was also owing in part to the pleasure and instruction which were afforded by his books of travel. Twice, as we have stated, Professor Silliman visited Europe, the interval between his journeys being nearly fifty years. Both these visits led to the publication of his observations in volumes which were widely read. The narrative of his earlier journey especially was received by the public with great delight. Few Americans then went abroad; and hardly any had published narratives of what they had seen. Mr. Silliman's volumes were fascinating to young and old,—and many were the testimonials which he received of the interest thus awakened in European institutions and manners. His Journal of a Tour to Canada was another contribution to the literature of the day.

But the general influence of Mr. Silliman must be attributed to his personal character rather than to any of what may be termed the accidental circumstances of his life. He was a man of vigorous understanding and sound judgment, led on, but never carried away, by an enthusiastic disposition, glowing and constant. With this was associated sterling integrity, which never harbored a selfish or dishonorable purpose, but rejoiced in doing and encouraging whatever was right. Every one could trust him. These fundamental traits were adorned by the outward qualities of affability and courtesy, or rather were expressed in manners at once so dignified and so kind that all with whom he came in contact were charmed at once, and on closer intercourse were bound to him as friends for life. Such friendships he never neglected or forgot. Even the sons and the grandsons of his early associates inherited a share in the regard which he had bestowed upon their parents. Blending with and ennobling all these virtues, was the child-like simplicity of his Christian faith.

A character like this shines the brighter the nearer it is seen. In his own family circle, Mr. Silliman has moved for years as a patriarch, surrounded by his descendants to the third and

fourth generation. The very house which he occupied has become historic, reflecting in its arrangements, its family portraits, its interesting mementos of absent friends, and its long shelves of books, the controlling mind which has dwelt there.

In the neighborhood and town where he resided, Mr. Silliman was peculiarly beloved and respected. "New Haven will not be New Haven without him," said more than one of his associates, as he heard of his death. His hand was always open to the needy. He was given to hospitality. He frequently took part in public meetings, and was actively concerned in all questions of local improvement. He rarely, if ever, failed to discharge his duties as a citizen at the polls, and was always ready to express his opinions on questions of public policy.

A whole-souled patriot, he viewed with the deepest interest the complications brought into the affairs of the country by the system of slavery. His general benevolence ever led him to sympathize with the oppressed, and the wrongs of the African touched him deeply. We cannot better indicate his feelings on this subject than by quoting a few sentences from his private journal under the date of April, 1850. After mentioning the death of the champion of what have been called "Southern rights," John C. Calhoun, his former pupil and friend, he gives a brief sketch of his character, concluding as follows:

"His public career has been highly distinguished. It is, however, very much to be regretted that he, many years ago, narrowed down his great mind to sectional views, and that he became morbidly sensitive and jealous of encroachment as regards the South, especially in reference to the protective tariff and to slavery. The former prompted his efforts for nullification, and the latter excited him to a vindication of slavery in the abstract. He, in a great measure, changed the state of opinion and the manner of speaking and writing upon this subject in the South, until we have come to present to the world the mortifying and disgraceful spectacle of a great republic—and the only real republic in the world—standing forth in vindication of slavery, without prospect of, or a wish for, its extinction. If the views of Mr. Calhoun, and of those who think with him, are to prevail, slavery is to be sustained on this great continent forever. I will not occupy my pages with any extended remarks upon this subject which is now agitating the national councils, and to a degree the nation itself. * * * It [the great question] is in better hands than man's; and I trust ultimately the colored men of all races on this continent will be received into the great human family as rational beings and as heirs of immortality."

As soon as the atrocities in Kansas revealed the determination of the advocates of slavery to perpetuate and extend that institution, even if they dissevered or destroyed the nation, Mr. Silliman came out with all his youthful ardor, and with the influence of his years and reputation, as the opponent of the slave-power. He thus became the object of personal defamation, even

in the Senate chamber at Washington; but he still remained firm, for he recognized in this war a slaveholder's rebellion. All the lofty sentiments of patriotism which were awakened in childhood, as he witnessed the commencement of national life, were intensified by this struggle to maintain the Union. He was sure that the nation would be purified by the conflict, and liberty established through all the land.

Mr. Silliman has always been remarkable for uniform good health, and in his later years manifested but slightly the encroachments of age. To the last, his form was as erect, his brow as serene, and his features as full of life and cheerfulness as in his earlier days; and his gait was only a little slower and more cautious.

He continued as usual until the middle of November just past, when he was for a few days quite unwell, probably as an immediate consequence of exposure to cold in attending an evening meeting in behalf of the Sanitary Commission. He had gradually, to appearance, regained nearly his former strength during the following week, and, on Wednesday, was intending to join the family Thanksgiving festival the next day at the house of his son-in-law, Prof. Dana. On the morning of that day, Nov. 24th, he awoke early, after a night of quiet rest, feeling stronger, as he said, than he had done for some days. He spoke with his wife of the many reasons there were for thankfulness, both public and private, dwelling at length upon the causes for national gratitude, especially in the recent re-election to the Presidency of a man who had proved himself so true, so honest, so upright in conducting the affairs of the government as Mr. Lincoln. As was his custom, while still in his bed, he offered up a short prayer, and repeated a familiar hymn of praise. In resuming his conversation, before rising, he spoke of the possibility of his attending the public services of the day, of the happiness of his home, of the love of his children, and, in strong terms of endearment, of his wife. Just as these his last words of love were uttered, there was a sudden change of countenance, a slightly heavier breath and he was gone. At the advanced age of 85, life to him was still beautiful; and not less so was its close. His sun set in the blessedness of the Christian's faith, to rise on a brighter morrow.

ART. II.—*Notice of the Explorations of the Geological Survey of California, in the Sierra Nevada, during the summer of* 1864.

IN the September number of this Journal (vol. xxxviii, page 298) a notice was inserted giving a sketch of the explorations of the Geological Survey of California in the High Sierra about the heads of King's and Kern rivers (latitude 36° 30′ to 37°). It was mentioned that Mr. King had, on the 6th of July, ascended a peak over 14,000 feet high, and that he was about to make an attempt to reach a point still higher, which was one of two in sight from, and higher than, the peak he had just been on. Since the last number of the Journal was issued, some further particulars have been received with regard to the peak ascended on the 6th of July, to which the name of Mt. Tyndall was given, and also a brief account of the attempt made to get on to the highest points.

The ascent of Mt. Tyndall was a very difficult and dangerous one; but the summit was reached without accident either to Mr. King or his companion, although the trip required that several nights should be spent at an elevation of over 12,000 feet, of course without fire, and with but scanty covering. From the summit of Mt. Tyndall, which is considerably over 14,000 feet high, there appeared two other peaks of equal elevation and two still higher, all within a distance of seven miles of this. Mr. King says, "of the two highest, one rose close by, being hardly a mile away; it is an inaccessible bunch of needles. The other was equally inaccessible from any point on the north or west side. The first-mentioned was about 150 feet above us, the other was six or seven miles distant and I should think fully 350 feet higher than the peak we were on. Within our field of view were five mountains over 14,000 feet and about fifty over 13,000. The five highest peaks are all in the eastern ridge. Owen's valley, a brown sage plain, lies 10,000 feet below on the one side, and Kern cañon, once the rocky bed of a grand old glacier, 4000 feet down on the other. About fifteen miles north of here, King's river cuts through the western ridge and turns at a right angle toward the plain. North of this point, again, the two great ridges unite in a grand pile of granite mountains, whose outlines are all of the most rugged and fantastic character. Twenty-five miles south, the high group ends, there (certainly for a breadth of sixty miles) forming one broad, rolling, forest-covered plateau, 8000 to 9000 feet in elevation."

"From Mt. Brewer to Kaweah Peak, the two culminating points of the western ridge, for a distance of fifteen miles, there is nothing that can be called a separate mountain: it is, rather, a great mural ridge, capped by small sharp cones and low ragged domes, all covered with little minarets. At one place the ridge

forms a level table; upon this lies an unbroken cover of snow. To the eastward, all this range, from King's river gateway to Kaweah Peak, presents a series of blank, almost perpendicular precipices, broken every mile or so by a bold granite buttress. Between these are vast snow fields, and also numberless deep lakes, of which the most elevated are frozen."

"The few *Pinus contorta*, and the groves of our new pine[1] have a peculiar black color, or, rather, dark bluish-green, which rather augments than relieves the desolate, naked aspect of things." "The only bits of bright color to break the solemn monotony of granite and snow are the blue lakes, which lie everywhere in the ancient glacier beds."

"Far away in the north there is a broad red band in the granite; other than that, all is gray. Beyond Owen's valley is a low desert range. The Coso and Inyo mountains are in plain sight."

"To the eastward, parallel ridges, one beyond another, lie stretched before you, rigid and stony. They have the same aspect as the mountains near Washoe, the same brown color, with red and yellow shadings."

"Nearly due north, fifty or sixty miles distant, rises a lofty group of mountains, which culminates in a white cone. This must be very high, for unbroken snow covers fully two thousand feet on the south side of the peak, while even the highest mountains in our group have no snow except on the north flank. These are probably the 'White Mountains.' I venture 14,600 feet as a guess at the probable elevation of the highest point in the group."

The White mountains, of which Mr. King speaks, in the extract cited above, lie just on the borders of California and Nevada, in about lon. 118°, lat. 37° 30′; they were distinctly visible to us from Mt. Dana and the other high peaks near Mono lake. It is doubtful whether the highest points are within the State of California; but they are probably very near the line on one side or the other. As it is by no means impossible, although I do not consider it probable, that some points of this range of mountains are higher than any yet measured or ascended by our parties, we still have to remain for some time in uncertainty as to whether California can claim the highest elevations in the country as within her borders; we can, at least, say that the highest measured ones are.

The ascent of Mt. Tyndall was a successful one, the party returning to camp at the end of the fifth day, with bones and barometers unbroken, although several very narrow escapes are briefly mentioned in Mr. King's notes.

It appears from Mr. Brewer's notes, that Mr. King's barometrical measurements of Mt. Tyndall would make that mountain above the height of Mt. Shasta, or over 15,090 feet, Mt. Shasta being 14,440. The height of Visalia, which is the plane of

[1] A new and not yet named species discovered by the Survey during this trip.

reference for all our observations this year in the High Sierra, has not yet been as accurately determined as it will be after a longer series of observations has been worked up, this place having been, during the past summer, and still continuing to be, one of Major Williamson's stations.

Not satisfied with the former attempt to reach the highest peak of the Sierra, Mr. King made another, starting from Visalia, July 14th, with the intention of following the Owen's lake and Visalia trail, and endeavoring to reach the culminating point from somewhere on this route. This trail leads up the Kaweah river, following up the South fork, from its junction with the middle fork of the same river. Full particulars of this reconnoissance have not yet come to hand; but from letters lately received, it appears that the base of the high point toward which his efforts were directed was, with difficulty, reached; but that the actual summit of the peak was found to be inaccessible—at least from any direction except that of Owen's valley, and probably not to be scaled from that side. The highest point attained by Mr. King on this trip was 14,369 feet above Visalia, which place is approximately 360 feet above the sea level, making the total height 14,729 feet, which is considerably above Mt. Shasta. But Mr. King was not, by estimate, within 300 or 400 feet of the summit, so that it appears that the elevation above the sea-level of this, so far as known, the culminating point of the Sierra Nevada cannot fall short of 15,000 feet.[2]

In the mean time, the main party, under Professor Brewer, made an attempt to work up the topography of this portion of the Sierra north to the Merced river, by keeping along at a high altitude, on the west side of the main ridge; but, after many perilous and laborious attempts, they found the ridges between the north and south forks of King's river to be impassable, so that they were obliged to cross the summit of the Sierra and descend into Owen's valley, in order to get north in that valley, and by crossing back on some other pass to reach the region of the head-waters of the San Joaquin. The pass by which the Sierra was crossed, going into Owen's valley, was about 11,600 feet high, all the region about the summit being exceedingly rough, sharp peaks of granite rising 2,500 feet or more above the pass on both sides. The party travelled for three days up Owen's valley, then turned and crossed the Sierra, by an old Indian foot-trail, to the head-waters of the San Joaquin, the summit of the pass being just about 12,000 feet high, and the mountains on each side 1,000 to 1,200 feet higher

From here the party made their way to Clark's ranch, between the Mariposa estate and the Yo-semite, through many privations and some perils, but without serious accident to life or limb.

[2] To this peak the name of Mount Whitney was given by Messrs. Brewer, King and Hoffmann.

The result of the summer's reconnoissance has been that a general idea is secured of the topography of a region about as large as Massachusetts, lying wholly within the State of California, and of which nothing whatever was known previously to this. That mountain peaks should be found in this part of the State, higher than any known to exist in the United States, is a discovery equally interesting and unexpected. The details of this exploration will furnish many facts of great geological and geographical interest. J. D. W.

Northampton, Mass., Oct. 15, 1864.

ART. III.—*On the Mineral Waters of Bath and other hot springs, and their Geological effects;* by Sir CHARLES LYELL, Bart.[1]

* * * * What renders Bath a peculiar point of attraction to the student of natural phenomena is its thermal and mineral waters, to the sanatory powers of which the city has owed its origin and celebrity. The great volume and high temperature of these waters render them not only unique in our island, but perhaps without a parallel in the rest of Europe, when we duly take into account their distance from the nearest region of violent earthquakes or of active or extinct volcanos. The spot where they issue, as we learn from the researches of the historian and antiquary, was lonely and desert when the Romans first landed in this island, but in a few years it was converted into one of the chief cities of the newly conquered province. On the site of the hot springs was a large morass, from which clouds of white vapor rose into the air; and there first was the spacious bath-room built, in a highly ornamental style of architecture, and decorated with columns, pilasters, and tessellated pavements. By its side was erected a splendid temple dedicated to Minerva, of which some statues and altars, with their inscriptions, and ornate pillars are still to be seen in the Museum of this place. To these edifices the quarters of the garrison, and in the course of time the dwellings of new settlers, were added; and they were all encircled by a massive wall, the solid foundations of which still remain.

A dense mass of soil and rubbish, from 10 to 20 feet thick, now separates the level on which the present city stands from the level of the ancient Aquæ Solis of the Romans. Digging through this mass of heterogeneous materials, coins and coffins of the Saxon period have been found; and lower down, beginning at the depth of from 12 to 15 feet from the surface, coins have been disinterred of Imperial Rome, bearing dates from the reign of Claudius to that of Maximus in the fifth century. Be-

[1] From the inaugural address at the opening of the meeting of the British Association at Bath, Sept. 14, 1864.

neath the whole, are occasionally seen tesselated pavements still retaining their bright colors, one of which, on the site of the Mineral-water Hospital, is still carefully preserved, affording us an opportunity of gauging the difference of level of ancient and modern Bath.

One of our former Presidents, Dr. Daubeny, has remarked that nearly all the most celebrated hot springs of Europe, such as those of Aix-la-Chapelle, Baden-Baden, Naples, Auvergne, and the Pyrenees, have not declined in temperature since the days of the Romans; for many of them still retain as great a heat as is tolerable to the human body, and yet when employed by the ancients they do not seem to have required to be first cooled down by artificial means. This uniformity of temperature, maintained in some places for more than 2000 years, together with the constancy in the volume of the water, which never varies with the seasons, as in ordinary springs, the identity also of the mineral ingredients which, century after century, are held by each spring in solution, are striking facts, and they tempt us irresistibly to speculate on the deep subterranean sources both of the heat and mineral matter. How long has this uniformity prevailed? Are the springs really ancient in reference to the earth's history, or, like the courses of the present rivers and the actual shape of our hills and valleys, are they only of high antiquity when contrasted with the brief space of human annals? May they not be like Vesuvius and Etna, which, although they have been adding to their flanks in the course of the last 2000 years many a stream of lava and shower of ashes, were still mountains very much the same as they now are in height and dimensions from the earliest times to which we can trace back their existence? Yet although their foundations are tens of thousands of years old, they were laid at an era when the Mediterranean was already inhabited by the same species of marine shells as those with which it is now peopled; so that these volcanos must be regarded as things of yesterday in the geological calendar.

Notwithstanding the general persistency in character of mineral waters and hot springs ever since they were first known to us, we find on inquiry that some few of them, even in historical times, have been subject to great changes. These have happened during earthquakes which have been violent enough to disturb the subterranean drainage and alter the shape of the fissures up which the waters ascend. Thus, during the great earthquake at Lisbon in 1755, the temperature of the spring called La Source de la Reine at Bagnères de Luchon, in the Pyrenees, was suddenly raised as much as 75° F., or changed from a cold spring to one of 122° F., a heat which it has since retained. It is also recorded that the hot springs at Bagnères de Bigorre, in the same mountain-chain, became suddenly cold

during a great earthquake which, in 1660, threw down several houses in that town.

It has been ascertained that the hot springs of the Pyrenees, the Alps, and many other regions, are situated in lines along which the rocks have been rent, and usually where they have been displaced or "faulted." Similar dislocations in the solid crust of the earth are generally supposed to have determined the spots where active and extinct volcanos have burst forth; for several of these often affect a linear arrangement, their position seeming to have been determined by great lines of fissure. Another connecting link between the volcano and the hot spring is recognizable in the great abundance of hot springs in regions where volcanic eruptions still occur from time to time. It is also in the same districts that the waters occasionally attain the boiling-temperature, while some of the associated stufas emit steam considerably above the boiling-point. But in proportion as we recede from the great centres of igneous activity, we find the thermal waters decreasing in frequency and in their average heat, while at the same time they are most conspicuous in those territories where, as in Central France or the Eifel in Germany, there are cones and craters still so perfect in their form, and streams of lava bearing such a relation to the depth and shape of the existing valleys, as to indicate that the internal fires have become dormant in comparatively recent times. If there be exceptions to this rule, it is where hot springs are met with in parts of the Alps and Pyrenees which have been violently convulsed by modern earthquakes.

To pursue still further our comparison between the hot spring and the volcano, we may regard the water of the spring as representing those vast clouds of aqueous vapor which are copiously evolved for days, sometimes for weeks, in succession from craters during an eruption. But we shall perhaps be asked whether, when we contrast the work done by the two agents in question, there is not a marked failure of analogy in one respect —namely a want, in the case of the hot spring, of power to raise from great depths in the earth voluminous masses of solid matter corresponding to the heaps of scoriæ and streams of lava which the volcano pours out on the surface. To one who urges such an objection it may be said that the quantity of solid as well as gaseous matter, transferred by springs from the interior of the earth to its surface, is far more considerable than is commonly imagined. The thermal waters of Bath are far from being conspicuous among European hot springs, for the quantity of mineral matter contained in them in proportion to the water which acts as a solvent; yet Professor Ramsay has calculated that if the sulphates of lime and soda, and the chlorids of sodium and magnesium, and the other mineral ingredients which they contain, were solidified, they would form in one year a

square column 9 feet in diameter, and no less than 140 feet in height. All this matter is now quietly conveyed by a stream of limpid water, in an invisible form, to the Avon, and by the Avon to the sea; but if, instead of being thus removed, it were deposited around the orifice of eruption, like the siliceous layers which encrust the circular basin of an Icelandic geyser, we should soon see a considerable cone built up, with a crater in the middle; and if the action of the spring were intermittent, so that ten or twenty years should elapse between the periods when solid matter was emitted, or (say) an interval of three centuries, as in the case of Vesuvius between 1306 and 1631, the discharge would be on so grand a scale as to afford no mean object of comparison with the intermittent outpourings of a volcano.

Dr. Daubeny, after devoting a month to the analysis of the Bath waters, in 1833, ascertained that the daily evolution of nitrogen gas amounted to no less than 250 cubic feet in volume. This gas, he remarks, is not only characteristic of hot springs, but is largely disengaged from volcanic craters during eruptions. In both cases, he suggests that the nitrogen may be derived from atmospheric air, which is always dissolved in rain-water, and which, when this water penetrates the earth's crust, must be carried down to great depths, so as to reach the heated interior. When there, it may be subjected to deoxydating processes, so that the nitrogen, being left in a free state, may be driven upward by the expansive force of heat and steam, or by hydrostatic pressure. This theory has been very generally adopted, as best accounting for the constant disengagement of large bodies of nitrogen, even where the rocks through which the spring rises are crystalline and unfossiliferous. It will, however, of course be admitted, as Professor Bischof has pointed out, that in some places organic matter has supplied a large part of the nitrogen evolved.

Carbonic acid gas is another of the volatilized substances discharged by the Bath waters. Dr. Gustav Bischof, in the new edition of his valuable work on chemical and physical geology, when speaking of the exhalations of this gas, remarks that they are of universal occurrence, and that they originate at great depths, becoming more abundant the deeper we penetrate. He also observes that, when the silicates which enter so largely into the composition of the oldest rocks are percolated by this gas, they must be continually decomposed, and the carbonates formed by the new combinations thence arising must often augment the volume of the altered rocks. This increase of bulk, he says, must sometimes give rise to a mechanical force of expansion capable of uplifting the incumbent crust of the earth; and the same force may act laterally, so as to compress, dislocate, and tilt the strata on each side of a mass in which the new chemical changes are developed. The calculations made by this eminent

German chemist of the exact amount of distention which the origin of new mineral products may cause, by adding to the volume of the rocks, deserve the attention of geologists, as affording them aid in explaining those reiterated oscillations of level—those risings and sinkings of land—which have occurred on so grand a scale at successive periods of the past. There are probably many distinct causes of such upward, downward, and lateral movements, and any new suggestion on this head is most welcome; but I believe the expansion and contraction of solid rocks, when they are alternately heated and cooled, and the fusion and subsequent consolidation of mineral masses, will continue to rank, as heretofore, as the most influential causes of such movements.

The temperature of the Bath waters varies in the different springs from 117° to 120° F. This, as before stated, is exceptionally high, when we duly allow for the great distance of Bath from the nearest region of active, or recently extinct, volcanos and of violent earthquakes. The hot springs of Aix-la-Chapelle have a much higher temperature, viz. 135° F., but they are situated within forty miles of those cones and lava-streams of the Eifel which, though they may have spent their force ages before the earliest records of history, belong, nevertheless, to the most modern geological period. Bath is about 400 miles distant from the same part of Germany, and 440 from Auvergne—another volcanic region, the latest eruptions of which were geologically coëval with those of the Eifel. When these two regions in France and Germany were the theaters of frequent convulsions, we may well suppose that England was often more rudely shaken than now; and such shocks as that of October last, the sound and rocking motion of which caused so great a sensation as it traversed the southern part of the island, and seems to have been particularly violent in Herefordshire, may be only a languid reminder to us of a force of which the energy has been gradually dying out.

If you consult the geological map of the environs of this city, colored by the Government surveyors, you will perceive that numerous lines of fault or displacement of the rocks are there laid down, and one of these has shifted the strata vertically as much as 200 feet. Mr. Charles Moore pointed out to me last spring, when I had the advantage of examining the geology of this district under his guidance, that there are other lines of displacement not yet laid down on the Ordnance Map, the existence of which must be inferred from the different levels at which the same formations crop out on the flanks of the hills to the north and south of the city. I have therefore little doubt that the Bath springs, like most other thermal waters, mark the site of some great convulsion and fracture which took place in the crust

of the earth at some former period—perhaps not a very remote one, geologically speaking. The uppermost part of the rent through which the hot water rises is situated in horizontal strata of Lias, and Trias, 300 feet thick; and this may be more modern than the lower part, which passes through the inclined and broken strata of the subjacent Coal-measures, which are unconformable to the Trias. The nature and succession of these rocks penetrated by the Bath waters was first made out by the late William Smith, in 1817, when a shaft was sunk in the vicinity in search for coal. The shock which opened a communication through the upper rocks may have been of a much later date than that which fractured the older and underlying strata; for there is a tendency in the earth's crust to yield most readily along lines of ancient fracture, which constitute the points of least resistance to a force acting from below.

If we adopt the theory already alluded to, that the nitrogen is derived from the deoxydation of atmospheric air carried down by rain-water, we may imagine the supply of this water to be furnished by some mountainous region, perhaps a distant one, and that it descends through rents or porous rocks till it encounters some mass of heated matter by which it is converted into steam and then driven upward through a fissure. In its downward passage, the water may derive its sulphate of lime, chlorid of calcium, and other substances from the decomposition of the gypseous, saline, calcareous and other constituents of the rocks which it permeates. The greater part of the ingredients are common to sea-water, and might suggest the theory of a marine origin; but the analysis of the Bath springs by Merck and Galloway shows that the relative proportion of the solid matter is far from agreeing with that of the sea, the chlorid of magnesium being absolutely in excess, that is, 14 grains of it per gallon for 12 of common salt; whereas in sea-water there are 27 grains of salt, or chlorid of sodium, to 4 of the chlorid of magnesium. That some mineral springs, however, may derive an inexhaustible supply through rents and porous rocks, from the leaky bed of the ocean, is by no means an unreasonable theory, especially if we believe that the contiguity of nearly all the active volcanos to the sea is connected with the access of salt water to the subterranean foci of volcanic heat.

Professor Roscoe, of Manchester, has been lately engaged in making a careful analysis of the Bath waters, and has discovered in them three metals which they were not previously known to contain—namely, copper, strontium, and lithium; but he has searched in vain for cæsium and rubidium, those new metals, the existence of which has been revealed to us in the course of the last few years by what is called spectrum analysis. By this new method, the presence of infinitesimal quantities, such as would

have wholly escaped detection by ordinary tests, are made known to the eye by the agency of light. Thus, for example, a solid substance such as the residue obtained by evaporation from a mineral water is introduced on a platinum wire into a colorless gas-flame. The substance thus volatilized imparts its color to the flame, and the light, being then made to pass through a prism, is viewed through a small telescope or spectroscope, as it is called, by the aid of which one or more bright lines or bands are seen in the spectrum, which, according to their position and color, indicate the presence of different elementary bodies.

Professor Bunsen, of Heidelberg, led the way, in 1860, in the application of this new test to the hot waters of Baden-Baden and of Dürkheim in the Palatinate. He observed in the spectrum some colored lines of which he could not interpret the meaning, and was determined not to rest till he found out what they meant. This was no easy task, for it was necessary to evaporate fifty tons of water to obtain 200 grains of what proved to be two new metals. Taken together, their proportion to the water was only as one to three million. He named the first cæsium, from the bluish-gray lines which it presented in the spectrum; and the second rubidium, from its two red lines. Since these successful experiments were made, thallium, so called from its green line, was discovered in 1861 by Mr. Crooks; and a fourth metal, named indium, from its indigo colored band, was detected by Professor Richter, of Freiberg, in Saxony, in a zinc ore of the Hartz. It is impossible not to suspect that the wonderful efficacy of some mineral springs, both cold and thermal, in curing diseases, which no artificially prepared waters have as yet been able to rival, may be connected with the presence of one or more of these elementary bodies previously unknown; and some of the newly found ingredients, when procured in larger quantities, may furnish medical science with means of combating diseases which have hitherto baffled all human skill.

While I was pursuing my inquiries respecting the Bath waters, I learned casually that a hot spring had been discovered at a great depth in the copper mine near Redruth, in Cornwall, having about as high a temperature as that of the Bath waters, and of which, strange to say, no account has yet been published. It seems that, in the year 1839, a level was driven from an old shaft so as to intersect a rich copper-mine at the depth of 1350 feet from the surface. This lode or metalliferous fissure occurred in what was formerly called the United Mines, and which have since been named the Clifford Amalgamated Mines. Through the contents of the lode a powerful spring of hot water was observed to rise, which has continued to flow with undiminished strength ever since. At my request, Mr. Horton Davey, of Redruth, had the kindness to send up to London many

gallons of this water, which have been analyzed by Professor William Allen Miller, F.R.S., who finds that the quantity of solid matter is so great as to exceed by more than four times the proportion of that yielded by the Bath waters.[2] Its composition is also in many respects very different; for it contains but little sulphate of lime, and is almost free from the salts of magnesium. It is rich in the chlorids of calcium and sodium, and it contains one of the new metals—cæsium, never before detected in any mineral spring in England; but its peculiar characteristic is the extraordinary abundance of lithium, of which a mere trace had been found by Professor Roscoe in the Bath waters; whereas in this Cornish hot spring this metal constitutes no less than a twenty-sixth part of the whole of the solid contents, which, as before stated, are so voluminous. When Professor Miller exposed some of these contents to the test of spectrum analysis, he gave me an opportunity of seeing the beautiful bright crimson lines which the lithium produces in the spectrum.

Lithium was first made known in 1817 by Arfvedson, who extracted it from petalite; and it was believed to be extremely rare, until Bunsen and Kirchhoff, in 1860, by means of spectrum analysis, showed that it was a most widely diffused substance, existing in minute quantities in almost all mineral waters, and in the sea, as well as in milk, human blood, and the ashes of some plants. It has already been used in medicine, and we may therefore hope that, now that it is obtainable in large quantities, and at a much cheaper rate than before the Wheal-Clifford hot spring was analyzed, it may become of high value. According to a rough estimate which has been sent to me by Mr. Davey, the Wheal-Clifford spring yields no less than 250 gallons per minute, which is almost equal to the discharge of the King's Bath or chief spring of this city. As to the gases emitted, they are the same as those of the Bath water—namely carbonic acid, oxygen, and nitrogen.

Mr. Warington Smyth, who had already visited the Wheal-Clifford lode in 1855, re-examined it in July last, chiefly with the view of replying to several queries which I had put to him; and, in spite of the stifling heat, ascertained the geological structure of the lode, and the exact temperature of the water. This last he found to be 122° F. at the depth of 1350 feet; but he scarcely doubts that the thermometer would stand two or three degrees higher at a distance of 200 feet to the eastward, where the water is known to gush up more freely. The Wheal-Clifford lode is a fissure varying in width from six to twelve feet, one wall consisting of elvan or porphyritic granite, and the other of killas or clay-slate. Along the line of the rent, which runs

[2] See for the analysis, the last volume of this Journal, p. 447.

east and west, there has been a slight throw or shift of the rocks. The vein-stuff is chiefly formed of cellular pyrites of copper and iron, the porous nature of which allows the hot water to percolate freely through it. It seems, however, that in the continuation upward of the same fissure, little or no metalliferous ore was deposited, but in its place, quartz and other impermeable substances, which obstructed the course of the hot spring, so as to prevent its flowing out on the surface of the country. It has been always a favorite theory of the miners that the high temperature of this Cornish spring is due to the oxydation of the sulphurets of copper and iron, which are decomposed when air is admitted. That such oxydation must have some slight effect is undeniable; but that it materially influences the temperature of so large a body of water is out of the question. Its effect must be almost insensible; for Professor Miller has scarcely been able to detect any sulphuric acid in the water, and a minute trace only of iron and copper in solution.

When we compare the temperature of the Bath springs, which issue at a level of less than 100 feet above the sea, with the Wheal-Clifford spring found at a depth of 1350 feet from the surface, we must of course make allowance for the increase of heat always experienced when we descend into the interior of the earth. The difference would amount to about 20° F., if we adopt the estimate deduced by Mr. Hopkins from an accurate series of observations made in the Monkwearmouth shaft, near Durham, and in the Dukinfield shaft near Manchester, each of them 2000 feet in depth. In these shafts, the temperature was found to rise at the rate of only 1° F. for every increase of depth of from 65 to 70 feet. But if the Wheal-Clifford spring, instead of being arrested in its upward course, had continued to rise freely through porous and loose materials so as to reach the surface, it would probably not have lost anything approaching to 20° F., since the renewed heat derived from below would have warmed the walls and contents of the lode, so as to raise their temperature above that which would naturally belong to the rocks at corresponding levels on each side of the lode. The almost entire absence of magnesium raises an obvious objection to the hypothesis of this spring deriving its waters from the sea; or if such a source be suggested for the salt and other marine products, we shall be under the necessity of supposing the magnesium to be left behind in combination with some of the elements of the decomposed and altered rocks through which the thermal waters may have passed.

Hot springs are, for the most part, charged with alkaline and other highly soluble substances, and, as a rule, are barren of the precious metals, gold, silver, and platinum, as well as of tin, copper, lead, and many others, a slight trace of copper in the Bath

waters being exceptional. Nevertheless, there is a strong presumption that there exists some relationship between the action of thermal waters and the filling of rents with metallic ores. The component elements of these ores may, in the first instance, rise from great depths in a state of sublimation or of solution in intensely heated water, and may then be precipitated on the walls of a fissure as soon as the ascending vapors or fluids begin to part with some of their heat. Almost everything, save the alkaline metals, silica, and certain gases, may thus be left behind long before the spring reaches the earth's surface. If this theory be adopted, it will follow that the metalliferous portion of a fissure, originally thousands of feet or fathoms deep, will never be exposed in regions accessible to the miner until it has been upheaved by a long series of convulsions, and until the higher parts of the same rent, together with its contents and the rocks which it had traversed, have been removed by aqueous denudation. Ages before such changes are accomplished, thermal and mineral springs will have ceased to act; so that the want of identity between the mineral ingredients of hot springs and the contents of metalliferous veins, instead of militating against their intimate relationship, is in favor of their being the complementary results of one and the same natural operation.

But there are other characters in the structure of the earth's crust more mysterious in their nature than the phenomena of metalliferous veins, on which the study of hot springs has thrown light: I allude to the metamorphism of sedimentary rocks. Strata of various ages, many of them once full of organic remains, have been rendered partially or wholly crystalline. It is admitted on all hands that heat has been instrumental in bringing about this re-arrangement of particles, which, when the metamorphism has been carried out to its fullest extent, obliterates all trace of the imbedded fossils. But as mountain-masses many miles in length and breadth, and several thousands of feet in height, have undergone such alteration, it has always been difficult to explain in what manner an amount of heat capable of so entirely changing the molecular condition of sedimentary masses could have come into play without utterly annihilating every sign of stratification, as well as of organic structure.

Various experiments have led to the conclusion that the minerals which enter most largely into the composition of the metamorphic rocks have not been formed by crystallizing from a state of fusion, or in the dry way, but that they have been derived from liquid solutions, or in the wet way—a process requiring a far less intense degree of heat. Thermal springs, charged with carbonic acid and with fluohydric acid (which last is often present in small quantities), are powerful causes of decomposition and chemical reaction in rocks through which they percolate.

If, therefore, large bodies of hot water permeate mountain-masses at great depths, they may in the course of ages superinduce in them a crystalline structure; and in some cases strata in a lower position and of older date may be comparatively unaltered, retaining their fossil remains undefaced, while newer rocks are rendered metamorphic. This may happen where the waters, after passing upward for thousands of feet, meet with some obstruction, as in the case of the Wheal-Clifford spring, causing the same to be laterally diverted so as to percolate the surrounding rocks. The efficacy of such hydro-thermal action has been admirably lilustrated of late years by the experiments and observations of Sénarmont, Daubrée, Delesse, Scheerer, Sorby, Sterry Hunt, and others.

The changes which Daubrée has shown to have been produced by the alkaline waters of Plombières, in the Vosges, are more especially instructive. These thermal waters have a temperature of 160° F., and were conveyed by the Romans to baths through long conduits or aqueducts. The foundations of some of their works consisted of a bed of concrete, made of lime, fragments of brick and sandstone. Through this and other masonry the hot waters have been percolating for centuries, and have given rise to various zeolites—apophyllite and chabazite besides others; also to calcareous spar, aragonite, and fluor spar, together with siliceous minerals, such as opal,—all found in the interspaces of the bricks and mortar, or constituting part of their rearranged materials. The quantity of heat brought into action in this instance in the course of 2000 years has, no doubt, been enormous, although the intensity of it developed at any one moment has been always inconsiderable.

The study, of late years, of the constituent parts of granite has in like manner led to the conclusion that their consolidation has taken place at temperatures far below those formerly supposed to be indispensable. Gustav Rose has pointed out that the quartz of granite has the specific gravity of 2·6, which characterizes silica when it is precipitated from a liquid solvent, and not that inferior density, namely 2·3, which belongs to it when it cools and solidifies in the dry way from a state of fusion.

But some geologists, when made aware of the intervention on a large scale, of water, in the formation of the component minerals of the granitic and volcanic rocks, appear of late years to have been too much disposed to dispense with intense heat when accounting for the formation of the crystalline and unstratified rocks. As water in a state of solid combination enters largely into the aluminous and some other minerals, and therefore plays no small part in the composition of the earth's crust, it follows that, when rocks are melted, water must be present, independently of the supplies of rain-water and sea-water which find

their way into the regions of subterranean heat. But the existence of water under great pressure affords no argument against our attributing an excessively high temperature to the mass with which it is mixed up. Still less does the point to which the melted matter must be cooled down before it consolidates or crystallizes into lava or granite afford any test of the degree of heat which the same matter must have acquired when it was melted and made to form lakes and seas in the interior of the earth's crust.

We learn from Bunsen's experiments on the Great Geyser in Iceland, that at the depth of only seventy-four feet, at the bottom of the tube, a column of water may be in a state of rest, and yet possess a heat of 120° Centigrade, or 248° F. What, then, may not the temperature of such water be at the depth of a few thousand feet? It might soon attain a white heat under pressure; and as to lava, they who have beheld it issue, as I did in 1858, from the southwestern flanks of Vesuvius, with a surface white and glowing like that of the sun, and who have felt the scorching heat which it radiates, will form a high conception of the intense temperature of the same lava at the bottom of a vertical column several miles high, and communicating with a great reservoir of fused matter, which, if it were to begin at once to cool down, and were never to receive future accessions of heat, might require a whole geological period before it solidified. Of such slow refrigeration, hot springs may be among the most effective instruments, abstracting slowly from the subterranean molten mass that heat which clouds of vapor are seen to carry off in a latent form from a volcanic crater during an eruption, or from a lava-stream during its solidification. It is more than forty years since Mr. Scrope, in his work on volcanos, insisted on the important part which water plays in an eruption, when intimately mixed up with the component materials of lava, aiding as he supposed, in giving mobility to the more solid materials of the fluid mass. But, when advocating this igneo-aqueous theory, he never dreamed of impugning the Huttonian doctrine as to the intensity of heat which the production of the unstratified rocks, those of the plutonic class especially, implies.

The exact nature of the chemical changes which hydrothermal action may effect in the earth's interior will long remain obscure to us, because the regions where they take place are inaccessible to man; but the manner in which volcanos have shifted their position throughout a vast series of geological epochs—becoming extinct in one region and breaking out in another—may, perhaps, explain the increase of heat as we descend toward the interior, without the necessity of our appealing to an original central heat, or the igneous fluidity of the earth's nucleus.

Art. IV.—*On the Nebular Hypothesis;* by David Trowbridge, A.M.

[Concluded from vol. xxxviii, p. 360.]

1. *The Breaking-up of the Rings.*

34. The process of cooling would still continue after the rings had separated. The loss of caloric from radiation would cause the rings to contract their dimensions; and this, the unequal density of different parts, and the extraordinary perturbations to which they would necessarily be subject in their motions, would cause a separation of the rings in certain weak places. When once broken into parts while the density of the ring was very small, even if the parts were not many, the case would be extraordinary in which the parts would be prevented from re-uniting into a single planet, owing to the very great perturbations to which their motions would be subject, as separate bodies. It is impossible at present to tell just how a ring would be resolved into a planet. A system of waves would probably be developed, owing to perturbations; and by an accumulation in one part, owing to the nature of the disturbing force, such as calculation shows might exist under certain conditions in the case of the rings of Saturn, and thus a considerable portion of the matter of the ring would be driven into one side, and this accumulation would be the center of attraction around which the planet would be formed.

35. We may now ask in what direction the planet thus formed would rotate? The direction of rotation would depend on circumstances. Let us take the breadth of the ring from which Uranus was formed, the same as the diameter of his sphere of attraction in Kirkwood's Analogy. The diameter of the sphere of attraction of Uranus, as given by Professor Kirkwood,[28] is 7·438. The inner radius is 2·558, and the outer one, 4·879. If we suppose the inner and the outer parts of the ring each to have the velocity due it according to Kepler's third law, then, the velocity of the inner part being called 1, that of the outer will be 0·8287; and the angular velocities of the same parts will be to each other as 1 to 0·4715. If every part of the ring have the same angular velocity, and that of the inner part be 1, the angular velocity of the outer part will be 1·457. If the inner and the outer parts have the same velocity, the angular velocity of the inner part being called 1, that of the outer part will be 0·6866. But none of these cases can obtain in any of the rings, but one or another will be approximated to, accord-

[28] This Journal, [2], xiv, p. 213.

ing to circumstances. The outer rings would approximate to the first case, and the inner ones, to the second case; while intermediate rings would approximate more or less, according to their position in the System, to the last case. In confirmation of this, we may refer to the case of Saturn's rings. It is demonstrated by mathematicians that the stability of the motions of those rings cannot exist unless the parts at different distances from the center of Saturn, have different angular velocities.[29]

But observation[30] shows that the stability of the motion of the Rings exists. We hence conclude that those parts of the Rings situated at different distances from the center of Saturn do have different angular velocities. But it is concluded from observation that the rings of Saturn are liquid;[31] we hence conclude that there is still greater necessity for the different parts of a gaseous ring situated at different distances from the center of attraction, to have different angular velocities; just as we have concluded in speaking of the separation of the rings from the equatorial parts of the great solar spheroid.

36. In the case of the first two or three rings abandoned by the primitive spheroid, their density would be relatively so small that the velocity of the outer parts would be regulated approximately by Kepler's third law. Such being the case, it is probable that the velocity of the inner parts will exceed that of the outer parts. When the rings break up to form planets, the parts will in a great measure retain the velocity which they had in the complete rings, and the consequence will be that the rotation of such planets will, at first, be performed in a direction opposite to that of their motions around the central body.[32] In consequence of the great friction of the different strata of the fluid planetary rings abandoned by the solar spheroid, the outer strata would, in most cases, probably, have a greater absolute velocity than the inner strata; and the planets formed from them would naturally at first take the direct motion of rotation, because the external parts having a greater velocity than the inner parts, the excess of velocity would give the planet a direct rotatory velocity. Even if some of the outer planets should prove to have a retro-

[29] Maxwell, *On the Stability of the Motion of Saturn's Rings*, pp. 3 and 45; Peirce in Gould's *Astronomical Journal*, ii. 17–18.

[30] Observation seems to show that the Rings of Saturn are gradually approaching the body of the planet; but it by no means follows that they will ever come in contact with the body of the planet. They have existed too long for us to suppose there is any probability that the Rings are to be precipitated upon the body of the planet before our eyes, as it were.

[31] See *Annual of Scientific Discovery* for 1852, p. 377.

[32] Professor Kirkwood has pointed this out in his article in this Journal, [2], xxxviii, 2–4; and he has also attempted to show that in certain cases the rotation will become direct from the action of other members of the System. See also, volume xxxvii, page 51, where Prof. Hinrichs has given a formula indicating the same thing that we have arrived at in the text.

grade rotatory motion, our reasoning would not lead us to conclude that such would be the case with many of them.

37. But are we certain that all the rings would break up; or if broken up, that each would form a single planet? In this part of our enquiry we cannot guess at it; nor will it do to be guided by the phenomena of the Solar System; for it is the phenomena of such a system of which we are to render a rational account. We must here be guided by strictly mathematical and philosophical deductions. Prof. Peirce, of Harvard University, in his investigation of the problem of the stability of the motions of Saturn's Rings, arrived at the remarkable conclusion that the dynamical equilibrium of the rings is preserved by the sustaining effect of the satellites in the very act of perturbation. He then makes the remark that the only place in the Solar System, among the primary planets, where we could, from the above conclusions, expect a permanent ring, is just within the powerful masses of Jupiter and Saturn.[33] But even here, he says, the ring could not exist, but must ultimately be destroyed. He says, "But had there been a ring at this part of the system, it must have been subject to such extraordinary perturbations that it would, in the course of time, have been vibrated up against the next inferior planet, *Mars;* and in this way have been broken into the asteroids. The orbits of planets, formed under such circumstances, must have been characterized by great eccentricity."

The above view of the formation of the Asteroids, needs some modification. According to Peirce's conclusions, drawn from his investigation of the problem of Saturn's Rings, a fluid ring might, perhaps, exist for some considerable length of time, within the orbit of Jupiter. Granting this to be true, it is difficult to escape the conclusion that the process of cooling, to which the ring would be subject, even if it were gaseous, would gradually reduce it to a condition approximating to that of the rings of Saturn at the present time; that is, a liquid state. The various conclusions, then, at which mathematicians have arrived, respecting the perturbations of those rings, will apply with greater or less force, according to circumstances, to the supposed ring within the orbit of Jupiter. Prof. Maxwell,[34] in his investigation of the problem of the motion of Saturn's Rings, discussed the case of a solid ring, a ring of disconnected particles, and a fluid (or more definitely a liquid,) ring. The first case he found to be one of unstable equilibrium. In the last two cases he found that if the perturbations to which the ring would be sub-

[33] Gould's Ast. Journal, vol. ii, p. 18. Also Annual of Scientific Discovery for 1852, p. 379.

[34] On the stability of the motion of Saturn's rings. An essay which obtained the Adams Prize for the year 1856, in the University of Cambridge, Eng. By J. Clerk Maxwell, M. A.

ject could be propagated around the ring in the form of waves, subject to particular conditions, the ring would be permanent; but if these particular conditions were not fulfilled, the ring would be destroyed. In the case of a fluid ring, (incompressible,) he found that in general a wide ring could not revolve every part of which had the same angular velocity.[35] In his recapitulation[36] he says, "We next took up the case of a flattened ring, composed of incompressible fluid, and moving with uniform angular velocity. The internal forces here arise partly from attraction and partly from fluid pressure. We began by taking the case of an infinite stratum of fluid affected by regular waves. I found the accurate values of the forces in this case. For long waves, the resultant force is in the same direction as the displacement, reaching a maximum for waves whose length is about ten times the thickness of the stratum. For waves about five times as long as the stratum is thick, there is no resultant force; and for shorter waves, the force is in the opposite direction to the displacement.

39. "Applying these results to the case of the ring, we find that it will be destroyed by the long waves unless the fluid is less than $\frac{1}{2}\frac{1}{2}$d of the density of the planet, and that in all cases the short waves will break up the ring into small satellites.

40. "Passing to the case of *narrow* rings, we should find a somewhat larger maximum density, but we should still find that very short waves produce forces in the direction opposite to the displacement, and that therefore, as already explained, these short undulations would increase in magnitude without being propagated along the ring, till they had broken up the fluid filament into drops. These drops may or may not fulfill the condition formerly given [in the essay] for the stability of a ring of equal satellites. If they fulfill the conditions, they will move as a permanent ring. If they do not, short waves will arise and be propagated among the satellites, with ever increasing magnitude, till a sufficient number of drops have been brought into collision, so as to unite and form a smaller number of larger drops, which may be capable of revolving as a permanent ring."

41. Basing our reasoning on the preceding results, we are led to the conclusion that under certain conditions—such as probably exist within the orbits of Jupiter and Saturn in the Solar System—the abandoned fluid ring may preserve its form for immense ages, and thus have time to cool down somewhat and approximate to the condition of an incompressible fluid, and then, with the changes already taken place, undergo the necessary transformation, to convert it into small, separate bodies. These bodies would continue to come into collision until the conditions of dynamical equilibrium were fully established, and thus en-

[35] Maxwell, p. 45. [36] Ib., p. 64.

able each body to move in its own independent orbit. Bodies so formed must vary in size from the largest of them to bodies too minute to be visible through our most powerful telescopes.[37] The original ring would necessarily have considerable width, and since it would be prevented from uniting into a single planet, any such zone of small planets would also be of considerable width, and the planets would range in mean distance from one limit of the broken ring to the opposite limit. The various disturbing causes would very probably cause the small planetary bodies to move in very eccentric (planetary) orbits, and the action of the short waves which would break up the ring, might, perhaps, cause the resulting planets to move in orbits considerably inclined to each other, and to the plane of the ecliptic.

42. The heterogeneousness of the materials composing the outer rings, and the great rarity of their physical condition, would, perhaps, cause some of the less dense portions, at the time of the breaking-up of the rings, owing to the action of the great disturbing forces to which their motions would be subject, to be projected to a considerable distance from either the outer or the inner parts of the rings, and such detached portions might never return to the parent masses, but would move around the central solar body in an elliptical orbit, having, perhaps, in some cases, considerable inclination to the plane of the equator of the rings from which they were projected. Such bodies would revolve around the sun as *Comets*. The theory of Central Forces[38] shows us that, when the distances from the central body and the initial velocity are given, the eccentricity is dependent on the angle of projection; that is, the angle formed by the radius-vector and the line of projection. Supposing comets to have been formed as we have just described, we see that the great eccentricity of their orbits results from the direction with respect to the radius-vector, in which the mass is thrown from the ring. Such comets would very probably move around the sun in the same direction as the planets. Those rings abandoned nearer the centre of the solar spheroid, would, very likely, be more symmetrical in the disposition of their materials, and consequently less likely to have any large collection of rare materials that could be projected from the rings. We hence conclude that those comets which have a comparatively short period of revolution, will not be very large and conspicuous objects when they approach their perihelia. We may further conclude that the last three or four rings would not throw off any comets. We hence conclude that periodic comets having a direct motion should be found in groups, perhaps, with periods of revolution a little greater or a little less than the planets to which they

[37] May we not in this way account for the existence of meteoric rings?
[38] See Math. Monthly, ii, 160.

nearest correspond in their motions. Retrograde comets, and a portion of those which have a direct motion, cannot very well be accounted for upon this hypothesis of the breaking-up of the rings, nor should we expect to find retrograde comets with short periods. If, as Laplace supposed, and which, according to our hypothesis, is not improbable, comparatively small quantities of nebulous matter exist in different regions of the universe, the Solar System may, in its motion through space, come into their immediate neighborhood at different times, and such nebulous matter would be drawn, by the attractive influence of the sun and planets, into the system, and it would appear in the form of comets—either retrograde or direct, according to circumstances—and meteors. All comets so formed would necessarily have a comparatively long period of revolution, unless (which might be the case in some instances) the perturbative influence of the planets should change their orbits. We may hence conclude that all, or nearly all, comets having a short period, will have a direct motion; and of the comets of long periods, the direct and retrograde should, considering all time, be nearly equally divided, so far as we can determine.

2. *The Planets and their Satellites.*

43. After the rings from which the planets were formed were broken up, and all the parts of any one ring reduced to a planet, such planet, as we have shown, would have a rotation on an axis. This axis would, necessarily, be a *natural* or *principal axis*. It would be a delicate adjustment to give a solid body, differing from a sphere, a rotation around a principal axis; but a fluid body would readily adapt itself to such an axis. We do not doubt but that Infinite Wisdom and Power could cause any solid body to rotate around a natural axis; but we have not the least evidence that Infinite Wisdom ever works in that special manner. God always adapts means to ends, so that all things are produced under the action of fixed natural laws. We, therefore, conclude that the planets, formed as required by the Nebular Hypothesis, should all be found to rotate on *natural axes*.

44. After the parts of the broken-up rings had united, and the resulting bodies commenced their rotatory motion, the cooling of the masses, in consequence of the radiation of heat, would cause their rotatory velocities to be increased, and this process would continue till, in most instances, a secondary ring would be abandoned by such planetary bodies.[39] The outer planets being larger and much less dense than the inner ones, would abandon *several* secondary rings. In reference to the number of these secondary rings cast off by each of the planets, all that we can

[39] We have attempted here to be guided by strictly reasonable conclusions, without any reference to the phenomena of the Solar System.

at present conclude on, with any degree of certainty, is, that the number should, upon the whole, increase from the sun outward. We cannot, at present, say that the outermost planet ought to have abandoned the greatest number of rings. These secondary rings would, in general, break up and form secondary planets, or satellites. Under certain conditions—such as Prof. Peirce has found to exist in the System of Saturn—a ring, or rings might remain entire, or at least, not break up into a single satellite. According to this view, rings, if they exist, must be found interior to several satellites. In every case—unless changed by disturbing forces—the planet should rotate on its axis in less time than is required for any ring or satellite to revolve around the primary. The same must hold in the case of the sun and the planets.

45. When the last ring has been abandoned by any one of the planets, the remaining part[40] must cool down and thus form a primary planet. The outer portions being exposed to the low temperature of space—at least 50° below zero Fah.—they will cool much more rapidly than the inner parts, and after the whole is reduced to a liquid, a crust would, comparatively soon, form around the liquid planet, and further radiation would take place very slowly compared with its former rate. A certain amount of heat would be received from the other bodies, particularly the sun, (whatever might be its condition), and finally a balance would be reached beyond which the planet would not cool,—or at most very slowly,—and thus the inner parts would remain liquid. The outer planets being from the beginning less dense than the others, a crust would, perhaps, be formed so comparatively early as to leave the mean density of the planet comparatively small. We should, therefore, look for an increase in mean density from the outermost planet to the innermost. We see no reason, however, to believe that any simple law regulates this variation of mean density from one planet to another.

46. It would at first seem as if the satellites of the primaries should follow the same law of rotation that the primaries themselves do; but we must recollect that the numbers representing the distances of the former, expressed in radii of the latter, may differ very considerably from the numbers representing the distances of the latter expressed in radii of the sun. Again, the rings from which the satellites were formed, were abandoned when the primaries were much reduced in temperature, and condensed, when compared with the condition of the primary rings

[40] In the Nebular Hypothesis, after a fluid body has abandoned all the rings possible, there *must* remain a central body of comparatively large dimensions, and of a mass far greater than the sum of the masses of all the rings separated. How would the author of the meteoric theory as given in the 204th number of the North American Review, account for the fact that our Solar System is constructed upon this principle?

when they were separated from the primitive solar spheroid. All the satellites will be much smaller than the primaries; and, being comparatively small bodies, they will cool down and become solid much sooner than the primaries will. In consequence of the forces acting on the planets, both primaries and secondaries, they will become spheroidal in shape as soon as the rings from which they are made are broken up and the parts united. The attraction of the primaries on the fluid satellites, will raise the fluid particles into great tidal-waves; and as the waves on opposite sides are slightly unequal, the result will be a retardation of the rotatory velocity of the secondaries. William Ferrel, Esq., of the Nautical Almanac Office, Cambridge, Mass., has given[41] a mathematical discussion of the retarding effect of the tides of the earth on its rotatory velocity. According to his conclusions, the retarding effect of the earth upon the moon's rotatory velocity, both acting under similar circumstances, would be between 500 and 600 times as great as that of the moon on the earth's. According to his results, it would amount to an entire circumference of the moon, or one period of rotation of the moon in a hundred years. Even if these numbers be wide of the truth, the investigation shows us that, in general, the retarding effect of the attraction of the primaries on their secondaries, when in a fluid state, is sufficient, in the course of immense ages, to reduce the periods of revolution and rotation of the latter to isochronism. We have, therefore, great reason to suppose that as a general rule the satellites turn on their axes but once during a revolution around their primaries.

47. If there exists a cosmical ether, as is at present pretty generally admitted, in order that it may remain spread throughout universal space, it is only necessary for it to possess an elasticity so great that the action of luminous bodies is sufficient to produce a mechanical action in it that will enable it to maintain its temperature and fluid condition under all circumstances. This cosmical ether being material in its nature, it would necessarily partake of the motion of those bodies with which it remains in contact for immense ages of time. In the Solar System, the motion of the ether around the sun would be in the general direction of all the planets. It would also be regulated in its motion

[41] Gould's Ast. Journal, iii. 138–141. Professor Ferrel informs me that some numbers which he employed he now knows to be about ten times too large, and hence his results should be diminished in nearly the same ratio. He found that in one century the moon retarded the rotatory motion of the earth's equator 87·44 miles. Dividing this by 10, to correspond more nearly with more recent data, we have about 8¾ miles for the retarding effect of the moon in a century. The effect of the earth on the moon he found to be 562½ times as great as that of the moon on the earth; and this will give—since the diameter of the moon is only about one-fourth of the diameter of the earth—between 500 and 600 miles for the retarding effect of the earth upon the moon's equater, if it be only partly fluid. If it be wholly fluid, it would be somewhere near 1000 miles per century.

partly according to the third law of Kepler; and thus the planets, whatever might be their distances from the sun, would scarcely be retarded by it in their annual revolutions.[42] Since the ether is supposed to maintain its temperature, and therefore elastic force, it would not gather into separate collections, leaving some parts without it; but it would only be slightly condensed around the different bodies of the universe.

48. If we calculate the principal radii of gyration of Jupiter, at the time when each secondary ring was abandoned, upon the same principle as that adopted in the case of the sun, viz: by neglecting the masses of the satellites, we shall find such radii, at the time when each satellite ring was separated, commencing with the nearest and taking them in the order of distance—the equatorial radius of Jupiter being the unit—to be as follows:

TABLE IV.

$k_1 = 1{\cdot}308;$	$a_1 = 6{\cdot}048;$
$k_2 = 1{\cdot}832;$	$a_2 = 9{\cdot}623;$
$k_3 = 2{\cdot}630;$	$a_3 = 15{\cdot}350;$
$k_4 = 4{\cdot}018;$	$a_4 = 26{\cdot}998;$

a_1, &c., being the distances of the satellites. If we compare the radii of gyration, k, with the mean distance of the satellites, a, we see that in every instance the latter is not far from six times the former. If we make a similar comparison for the primary planets, using Table II, we see that the radius of gyration is only about the one-hundredth of the mean distance of the planet from the center of the sun. We hence conclude from this that the density of the primitive solar spheroid increased much more rapidly toward the center, than the primitive Jovian spheroid. In other words, the solar spheroid was relatively much more dense about the central parts than Jupiter. This we had reason to expect, since, according to the hypothesis, the ring from which the Jovian system was formed was already somewhat condensed when it was abandoned, and the general density much more nearly uniform than that of the solar spheroid; and hence the secondary rings were much denser relatively to the Jovian spheroid than was the ring from which Jupiter was formed relatively to the solar spheroid. This, it would seem, is a strong proof of the truth of the nebular hypothesis. The mass of Jupiter is 6000 times as great as the sum of the masses of his satellites; so that neglecting the masses of the satellites in comparison with the mass of Jupiter we neglected only the small fraction $\frac{1}{6000}$th. I supposed Jupiter homogeneous in finding his principal radius of gyration.

The periods of rotation of Uranus and of Neptune being un-

[42] This conclusion differs from the basis assumed by Prof. Hinrichs in his article on the Age of the Planets, this Journ., [2], xxxvii, 36-56.

known, we cannot compute the radii of gyration of the planets at the time when the satellite rings were abandoned, as we have done in the case of Jupiter. And if Bessel's mass of the rings of Saturn ($\frac{1}{11}$th of that of Saturn) be admitted, it is probable that this, and the sum of the masses of the satellites, will make a fraction too great to be neglected. The same is true of the earth and moon.

49. Let us now collect together in this article the various facts which ought to be found to exist in the Solar System, according to the preceding view of the nebular hypothesis.

1st. The planets should all move in the same direction around the sun. 2nd. Some comets whose distances correspond with those of the planets within moderate limits, should have the same direction as the planets. 3d. The orbits of the planets should not be much inclined to each other. 4th. All the planets, except, perhaps, a few of the outer ones, should turn on their axes in the same direction as that in which they move around the sun. 5th. The satellites should revolve around their primaries in the same direction in which the latter turn on their axes. 6th. The periods of revolution and rotation of the satellites, should, in many cases, at least, be equal. 7th. The sun must rotate on an axis in the same direction as that in which the planets revolve around him. 8th. The outer planets of the system should be larger than the inner ones. 9th. The outer planets should have the greater number of satellites. 10th. The outer planets should rotate on their axes in less time than the inner ones. 11th. The outer planets should in general have greater masses than the inner ones. 12th. The planets should, upon the whole, increase in density as they are found nearer the sun. 13th. The satellites should be less than their primary, and the planets less than the sun, both in bulk and mass. 14th. All the members of the Solar system should be composed of similar materials.

50. We have thus, in the preceding pages, attempted to give a connected view of the nebular hypothesis, uncompared, except in a few cases, with the real phenomena of nature. In what follows, we shall show that the phenomena of the sidereal heavens, and of the Solar System, agree very closely with the preceding deductions.

3. *A Comparison of the preceding Theory with the Phenomena of Nature.*

51. The origin of material existence is at present a mystery to the human mind. To say that it was created by the Deity is an assertion that conveys to the understanding something of which we have no definite conception. On the other hand, to say that it has always existed is equally unsatisfactory, since the human mind always looks toward a beginning of everything.

But with such speculations, we have here nothing to do. The nebular hypothesis supposes matter and all physical forces to exist; and we have only to carry back our speculations to the most ancient and chaotic state in which it is possible for us to suppose matter to exist. I have supposed matter in its earlier chaotic condition to have been necessarily heterogeneous in structure. Others have different views.[43] Even if it[44] were at first, or at any time, perfectly homogeneous, but not of symmetrical form, it would soon become heterogeneous, because various centers of attraction would be established, around which matter would accumulate; and thus the change in the different parts would generate a rotatory motion. Thus, under almost any conceivable—certainly probable—circumstances, the nebulous mass must be considered as having sufficient potential energy to give it a motion of rotation. But we must not be confined to these speculative views. "Every well-trained philosophical judgment is accustomed to observe illustrations of the most sublime phenomena of creation in the most minute and familiar operations of the Creator's laws, one of the most characteristic features of which consists in the absolute and wonderful integrity maintained in their action whatsoever be the range as to magnitude or distance of the objects on which they operate. For instance, the minute particles of dew which whiten the grass blade in early morn, are, in all probability, moulded into spheres by the identical law which gives to the mighty sun its globular form."[45] "It is remarkable of physical laws that we see them operating on every kind of scale as to magnitude, with the same regularity and perseverance. . . . . Two eddies in a stream fall into a mutual revolution at the distance of a couple of inches, through the same cause that makes a pair of suns link in mutual

[43] "I regard the gaseous as not the *ultimate* form of matter, but that in its ultimate state it was *absolutely* imponderable and perfectly elastic, and therefore it completely filled, absolutely, infinite space, and that the myriads of Solar vortices were developed out of this perfectly homogeneous imponderable fluid in a manner as definite and orderly as that which *regulates the production* of a plant or an animal on the earth."—*Dr. Joel E. Hendricks.*

[44] Some writers seem to doubt the existence of nebulous matter, because the telescope continues to resolve the nebulæ. But comets are evidently in a nebulous state, and the existence of these shows us that matter may assume the nebulous condition. It is a fact that the evidence derived from observation, that nebulous matter, in large quantities, exists, is by no means conclusive. In fact, the truth of the nebular hypothesis cannot depend on our knowledge of the existence of nebulous matter in large quantities. It is sufficient to show that matter in such a state can and does exist in *any* quantity, and after that we must look to the general and particular phenomena of nature. It is scarcely probable that we shall ever learn, at least for ages to come, directly from observation, that large quantities of matter exist in a nebulous condition. But the general appearance of starry clusters, may indicate the origin whence they were derived. The appearance and arrangement of the planetary system are more conclusive in reference to the origin whence that system was derived.

[45] Mr. Nasmyth. See Annual of Scientific Discovery for 1857, p. 187.

revolution at the distance of millions of miles. There is, we might say, a sublime simplicity in this indifference of the grand regulations to the vastness or the minuteness of the field of their operations."[46] We thus may learn from the minuter operations of nature of those grand revolutions which we have reason to conclude have taken place in past ages of duration. Binary, ternary, and multiple stars, in some, perhaps many, cases, may be the resulting motion of two or more stars—I should more properly say primitive stellar spheroids, coming so near together that their atmospheres came in contact and thus gave them a motion around their common center of gravity.[47]

52. All apparent motion in the region of the so-called fixed stars, is very slow; so slow, indeed, that in most instances it requires several years to elapse before *any* motion becomes apparent. Certainly, in most cases the nebulæ are situated far beyond the region of the visible stars. How much slower, then, than the stars, must the nebulæ appear to change their relative parts or positions in the heavens! We must, therefore, expect little from the motion of nebulæ or even clusters, in confirmation of the nebular hypothesis. We must look to the general conformation of nebulæ and of clusters. Lord Rosse has within a comparatively few years, by means of his great reflectors, shown that many of the nebulæ are of the *spiral* form. They appear as if they possess a rotatory motion on an axis, and this motion has so far increased as to project some parts of the nebulæ tangentially, and thus to break up the spheroidal form, causing parts of the nebulæ to fly off in one or more streams. The general principles which would lead to such a result, we have attempted to give from Arts. 3 to 12 inclusive. Mr. Nasmyth has also given[48] an exposition of the principles, though he has not based his explanations on mathematical formulæ and deductions as I have attempted to do. Many nebulæ present that appearance which we suppose our Solar System to have presented before the first planetary ring was abandoned, viz: a projected circular or elliptic appearance, not very bright in the outer portions, but increasing in luminosity with considerable rapidity near the center.

53. Some observations and deductions of a very interesting character, have recently been made. Many years ago, Sir William Herschel concluded, from a comparison of a figure of the nebula in Orion, made by Huygens many years before, with a figure of his own drawing, that some parts of the great nebula —(cluster, perhaps we should now say, since it has been par-

[46] Vestiges of Creation. Harper's Ed., p. 17.

[47] I am unable to ascertain in what direction, if any definite one, double stars revolve around their common center of gravity. Who will give the required information? It is worthy of attention.

[48] Ann. of Sci. Disc. for 1852, pp. 187-8.

tially resolved since Herschel's day)—have changed. It is very probable that no such change has taken place; but that the supposed variation came from a defect in Huygen's representation, and from a difference in the telescopes employed. But it seems now quite certain that some nebulæ have changed. The nebula around *Eta Argus*, seems to be one of that character.[49] Also several others have been discovered in which some change seems to have taken place. Accurate micrometrical measurements and photographic representations only can here serve for the basis of positive knowledge in this interesting department of astronomy. It would seem that such patches of nebulæ, if, indeed, they *are* nebulæ, proper, cannot be very extensive, since they must be comparatively near our system. We must now leave this interesting part of our subject, since this, alone, could never, so far as we can see, furnish us the necessary data to enable us to say whether the God of Nature followed one or another method in the process of world-making. We must descend to the general and some of the particular phenomena of the Solar System, to enable us to draw a definite conclusion on this subject.

54. We know from observation that all the primary planets revolve around the sun in the same direction, and in nearly the same plane. The sun rotates on an axis in the same direction in which the planets revolve around him, from west to east. The period of the sun's rotation is about 25⅓ days, which is about $\frac{7}{24}$ths of the periodic time of Mercury. According to our theory, no planet will ever be discovered so near the sun that its period of revolution will be less than the period of the sun, viz: 25⅓ days, if this number be accurately determined. This is contrary to the supposed discovery of Mr. Lescarbault.

The inclination of the orbit of Neptune, as we have shown, corresponds very approximately with that of the invariable plane of the Solar System, as our theory requires. We also find that the distance between the planetary orbits increases with the distance from the sun. The following table will show

TABLE V.

Planet.	Mean dist.	Dist. to next Ex. Orbit.	Diam. of sphere of attraction.
Mercury,	0·3870981	0·336235	0·200979
Venus,	0·7233306	0·2766684	0·383390
Earth,	1·0000000	0·5236923	0·521348
Mars,	1·5236923	1·544983	0·779537
Ast. Planet,	3·068675	2·134101	0·968693
Jupiter,	5·202776	4·336010	4·876551
Saturn,	9·588786	9·643604	8·618608
Uranus,	19·18239	10·857110	7·437871
Neptune,	30·03950		

[49] This Journal, [2], xxxvii, 294-5.

the difference between the mean distances of the planets, in terms of the mean distance between the earth and sun; and also the breadth of the primitive rings, according to Kirkwood's Analogy.[50] We have at present no method probably more accurate than Kirkwood's to determine the breadth of the primitive rings.

We thus see that, generally speaking, there is an increase of distance between the orbits of the planets, and also in the breadth of the rings abandoned by the sun; and yet it is not absolutely the case in every instance; nor is the increase according to any simple law, as we had occasion to point out from what was assumed to be the probable condition of the primitive solar spheroid.

55. If we compare the masses and the densities of the planets, we shall see here, also, a general increase in the masses, and a decrease in the densities, as we get farther from the sun, as can be seen in the following table:

TABLE VI.

Planet.	Magnitude.	Mass.	Density.
Mercury,.....	0·0595	0·0729	1·225
Venus,.....	0·9960	0·9101	0·908
Earth,.....	1·0000	1·0000	1·000
Mars,.....	0·1364	0·1324	0·972
Ast. Planet,.....		0·2869 [51]	
Jupiter,.....	1491·0000	338·7180	0·227
Saturn,.....	772·0000	101·3640	0·131
Uranus,.....	86·5000	14·2510	0·167
Neptune,.....	76·6000	18·9000	0·321

We see from this table what was concluded from theory to be very probable, viz: that in general the volumes and masses increase from the sun to a maximum—which is reached in Jupiter—and then decrease to the extremity of the system. The fact that there is no simple law perceptible which regulates the increase and decrease of the numbers in the two preceding tables, shows that the condition of the primitive solar spheroid was far from being symmetrical. The fact that the earth is more dense than Venus may be thus explained. The difference in the condition of the Earth-ring and the Venus-ring could not have been very considerable. But the former, after it united into a single spheroid, threw off a satellite-ring, taking off the rarer part and leaving the denser, so that the ultimate mean density of the earth became a little greater than that of Venus. There seems to be some peculiarity about Saturn in respect to density that cannot be easily accounted for.

[50] This Journal, [2], xiv, 213.

[51] According to Kirkwood's Analogy, this Journal, [2], xiv, 213.

56. In examining the secondary planets, we see a general increase in number to each primary planet, as we go from the sun to the extremity of the system. The Earth has 1, Jupiter 4, Saturn 8, Uranus 6 and perhaps 8, Neptune 2 that have probably been seen. We cannot tell with certainty the number of satellites belonging to each of the last two planets, owing to the difficulty of observing such bodies at so remote distances. The satellites of Jupiter are distributed in the Jovian system very similarly to the primary planets in the Solar System. We find the masses to increase as the distance from the center of Jupiter increases, till we arrive at the third satellite, where we reach the maximum. The fourth satellite is the second in mass. The third satellite is also the largest, and the fourth the next in size, but the second, although it is of greater mass, yet is of smaller size than the first. The satellites of Saturn follow a similar general law. *Titan*, the 6th in the order of distance from the primary, is the largest, and *Japetus*, the most distant of them all, is next in size. Of the others we cannot so well judge, but it seems not improbable that they decrease in size as they are nearer the primary. We know still less of the satellites of Uranus, but the second and fourth, so-called, are very probably the largest. But within the second, (in the order of distance according to Sir William Herschel's discoveries), two, and possibly three, exist. We thus see it to be a general principle of the planetary system that the largest bodies are within some, and without some of the orbits of the others. We also observe this fact in reference to some of the secondary planets, viz: if we divide the distance of the remotest satellite of any primary, from the center of the planet, by the diameter of the sphere of attraction (which we shall consider to be the same as the breadth of the ring, or the diameter of the primitive planet,) we shall find that, in general, the quotient decreases from the earth to Uranus. We may therefore conclude that the outer planets had to condense much more than the inner ones before a satellite-ring was abandoned. It is a fact, also, that, so far as observation has determined, the satellites rotate on their axes in the same direction and in the same time in which they revolve around their respective primaries. All these motions are in the direction of the rotation of the primaries.

57. The rings of Saturn offer a living example of the primitive secondary rings. They open to us, in a measure, the nature and constitution of the primitive rings, both the primary and secondary. These rings have been retained as such, according to Prof. Peirce, by the attraction of the satellites. Should these rings entirely break up, they would probably form asteroid satellites. Since the rings of Saturn are very thin in comparison with their width, we conclude that the primitive planet was very

much flattened about the poles. If the rings of Saturn are fluid, they exist as such in consequence of the friction of their particles.

58. It is an interesting fact, and one confirmatory of our theory, that the rings of Saturn have a longer period of rotation than the planet itself, as required by the nebular hypothesis. According to the observations of Sir William Herschel, the rings of Saturn revolve around the planet in the space of $10^h\ 32^m\ 15^s$.[52] The period of rotation of Saturn is $10^h\ 29^m\ 17^s$.[53] We thus see that the rings require $2^m\ 58^s$ longer time to revolve about Saturn, than the latter does to turn on his axis. Here again we appear to have obtained conclusive evidence in favor of the nebular hypothesis. Had the period of Saturn's rotation been greater than that of the rings, it would have been very difficult to reconcile it with the nebular hypothesis.

59. The rings of Saturn are either fluid or composed of unconnected particles. According to Maxwell, even a fluid ring would be broken up into small satellites; but it does not follow that these satellites may not still be fluid and unite again into a ring—or perhaps several nearly concentric rings—after having been separate a sufficient length of time to restore the equilibrium by counteracting the disturbances to which the rings are subject. We are thus reduced to the very probable conclusion that the rings of Saturn are fluid (liquid). We are thus carried back one great step toward the gaseous condition from which we have supposed all the planets and their satellites to have been developed.

60. Observation shows that the satellites of Uranus revolve in a retrograde order. But we do not know in what direction, nor in what time, the planet rotates. If it should finally prove to be the case that the satellites of Uranus are in the plane of the planet's equator, and revolve in a direction opposite to that of the rotation of the planet, it will be very difficult, if not impossible, to reconcile the two motions with the nebular hypothesis. But should the axis of the planet be found to have an inclination to the plane of the orbits of the satellites differing considerably from a right-angle, the difficulty will not be insuperable.[54] But since the nebular hypothesis accounts so satisfactorily for so many of the phenomena of the solar system, we may venture to predict that the rotation of the planet will be found to be retrograde, or closely approaching that direction, if we may so express ourselves. But our theory, as we have

[52] Grant's Hist. of Phys. Astr., p. 262.

[53] Herschel's Outlines, Art. 514.

[54] By giving an artificial globe a motion of rotation on its axis, and then reversing the poles, we can easily see how readily the direction of rotation is changed to the opposite direction. The same primitive impetus carries it in the opposite direction.

shown, requires that the outer planets rotate in a retrograde direction. Kirkwood's Analogy seems to require that Uranus should occupy about 37 hours in performing one rotation. Is it possible that a retrograde motion will influence the time of rotation of the planet? It would seem that some of the great telescopes, either in this country or in Europe, ought to be able to settle the question of the rotation of Uranus. I have not attempted to account for the great difference between the greatest and least diameters of Mars. A mere supposition could be of no advantage.

61. The following table gives the time of rotation of the several planets so far as known:

TABLE VII.

Planet.	Time of rotation.			Diam. of sphere of attraction.
	h.	m.	s.	
Mercury, ...	24	5	28	0·200979
Venus, ...	23	21	21	0·383390
Earth, ...	23	56	4	0·521348
Mars, ...	24	37	22	0·779537
Jupiter, ...	9	55	26	4·876551
Saturn, ...	10	29	17	8·618608

By the above table we see that, roughly speaking, the time of rotation diminishes as the sphere of attraction increases. The original impulse of rotation communicated to a planet, by the ring from which it was formed, at the time of its breaking-up, would be dependent on the difference between the velocity of those parts of the ring lying upon the opposite sides of the orbit of the future planet; or, in other words, upon the difference of the living force of each part. This difference would not depend on the absolute velocity of either part, but rather upon the relative distance of the two parts from the sun. We hence see that since the outer primitive rings were wider than the inner ones, we should expect a more rapid rotatory motion in the outer planets.[55] Since Saturn has the greatest sphere of attraction we should at first expect that planet to have the shortest period of rotation. But such is not necessarily the case. If this whole planetary ring had condensed into a single body, such might and probably would have been the case. But every time a planet abandoned a ring to form a satellite, it, in effect, lengthened the period of rotation of the future planetary spheroid. Jupiter abandoned only four rings of which we have any knowledge, and the united mass of them forms only the $\frac{1}{6000}$th of the mass of Jupiter; while Saturn abandoned no fewer than eleven rings, three of which still remain. According to Bessel, the mass of

[55] Since the outer rings were less dense than the inner ones, the planets would have farther to contract than the inner ones, and this also would increase the rotatory velocity.

the remaining rings alone equals the $\frac{1}{11\frac{1}{8}}$th of the mass of Saturn. Even now the period of Saturn's rotation exceeds that of Jupiter by only $3^m\ 51^s$.

62. Humboldt has called attention[46] to the fact that the group of Asteroids divides the planets into two classes, those within and those without the zone of small planets, each class being marked by certain distinctive features. The outer group, "Neptune, Uranus, Saturn and Jupiter, are distinguished by considerable volume and slight density; the time of rotation about their axes is about ten hours, and from this there results in these bodies considerable flattening; moreover, of the 22 satellites of the solar system, 21 pertain to this group. The four *inferior* planets, Mars, the Earth, Venus and Mercury, have, on the contrary, much smaller volumes and much greater density; they revolve on themselves in nearly twenty-four hours, are but little flattened, and possess, among all the four, but one satellite, the Moon." Let us now see what account we can give of these peculiarities. We have already shown that according to Prof. Peirce's deductions a planetary-ring might exist for a great length of time just within the orbit of Jupiter; the same influence that would sustain a ring where the asteroids now exist, would tend to sustain one where Mars now revolves; and the same influence would extend to the orbits of the Earth and of Venus; and each of these latter bodies would lend their influence, and would thus influence the Mercurial-ring. These interior rings (as we will call them, being interior to the Asteroids,) being thus sustained for a great length of time, would loose much of their heat, and thus become condensed considerably perhaps, as have the rings of Saturn. When such rings broke up and assumed the spheroidal form, they would be much less likely to abandon satellite rings, than those planets whose rarity was greater. We have already shown that the mean density of those planets nearer the sun should be found to be greater than that of the outer planets; we now refer to the influence above mentioned to account for the sudden increase of mean density within the orbit of Jupiter; for evidently the mean density of a planet formed from a ring considerably condensed, would be greater than one formed from a ring less so; for a crust would form around one approximately as soon as around the other. The period of rotation of such planets would be greater than that of others, since they would not have so far to contract. The times of rotation would be approximately equal, since they would contract nearly equally.

63. The existence of 80 known Asteroids between the orbits of Mars and Jupiter, seems to confirm the view which we have given of their formation, in a preceding part of this paper. If we divide 360° by 80, we find an average of one Asteroid for

[46] Cosmos, iv, 422, Bohn's ed.

every $4\frac{1}{2}°$, making approximately a ring of these small bodies. But we cannot for a moment entertain the idea that nearly all these bodies have been discovered. They are very small and necessarily difficult to detect; and judging from what we already know of the size of these small bodies, we must conclude that many more exist, much the larger share of which are too small to be detected by our best telescopes. The most rational theory of meteoric stones, is that which regards them as minute asteroids. The whole group, many thousands very probably, perhaps millions—between the orbits of Mars and Jupiter, must have very eccentric orbits, and be subject to extraordinary perturbations in their motions, and it is highly probable that many of them become satellites of some of the planets, and finally come in contact with their surfaces as meteoric stones. It is certainly a little curious that the orbits of the known Asteroids intersect. The mean width of the whole zone so far as known, lies between the limits 2·145 (Feronia) and 3·452 (Maxamiliana) giving a breath of 1·307, which is rather greater than the diameter of the sphere of attraction of Kirkwood's Asteroid-planet.

At present we shall add nothing respecting comets, as Prof. Kirkwood has called attention, in several places, to the orbits of these bodies.

Hector, N. Y., Nov. 11, 1864.

ART. V.—*On Brushite, a new mineral occurring in Phosphatic Guano;* by GIDEON E. MOORE, Ph.B.

(Communicated to the California Academy of Sciences, Sept. 5th, 1864.)

IN the spring of the present year, I received, through the kindness of Wm. E. Brown, Esq., of Mare Island in this State, a specimen of a mineral discovered by him in a cargo of phosphatic guano at Camden, N. J. The locality from which it was derived is not known,[1] and, though letters of inquiry have been sent to the parties to whom the cargo was originally consigned, no reply has been received up to this date. The texture and appearance of the guano would, however, point to some one of the Carribean islands, and more particularly to the island of Sombrero as its probable source. It is very probable that the mineral may be recognized among the crystallized products occurring in other guano deposits.

In the specimen in my possession, the mineral occurs filling seams in the guano, varying from $\frac{1}{8}$ to $\frac{1}{4}$ of an inch in width. The matrix itself is of the variety known as rock guano. It

[1] In a letter from Mr. Moore, dated San Francisco, Nov. 13th, 1864, he states that he has ascertained the locality of the new mineral to be Avis Island in the Carribean Sea.—EDS.

possesses an oölitic structure and a brownish white color, interspersed with small spots of pure white.

The mineral is in the form of small but very perfect and brilliant crystals, with a cleavage in the direction of their greatest length nearly equal to that of selenite, the laminæ being also slightly flexible, as in the case of the latter species. Hardness, 2·25. Specific gravity, 2·208 (mean of two determinations). Color yellowish white. Transparent. Lustre vitreous, splendent, inclining to pearly on the cleavage faces.

When heated in a closed tube before the blowpipe, it whitens and gives off water at an incipient red heat. In the platinum forceps, it fuses with intumescence at about 2 on von Kobell's scale, tinging the flame with the peculiar green characteristic of phosphoric acid. The button formed by fusion crystallizes on cooling, showing numerous brilliant facets. It readily dissolves, even in coarse crystals, in dilute nitric and chlorhydric acids.

A qualitative analysis revealed the presence of lime, phosphoric acid and water, with barely discernible traces of magnesia and alumina.

The quantity of mineral at my disposal was very small, scarcely exceeding one gram in weight. In each of the two following analyses, the water was determined in 0·2 gram, the remaining 0·3 gram being employed in the determination of the lime and phosphoric acid. The result was as follows:

	1.	2.
Lime,	32·65	32·73
Phosphoric acid,	41·50	41·32
Water,	26·33	26·40
	100·48	100·45

These figures agree exactly with the composition of the neutral tri-basic phosphate of lime, $2CaO, HO, PO_5$, with the addition of four equivalents of water of crystallization ($2CaO, HO, PO_5 + 4aq$), viz:

$2CaO$,	56·26	= 32·59
PO_5,	71·36	41·34
HO	9·00	
$4aq$,	36·00	26·07
	172·62	100·00

In the polarizing microscope, the mineral shows a vivid succession of colors. A sample has been sent to Prof. J. D. Dana, who has kindly undertaken the study of its crystallographic characters, and I hope in a short time to be able to communicate the results of his investigations to the Academy.

It is with great pleasure that I dedicate this species to Prof. G. J. Brush, of Yale College, to whose unwearied zeal and efficient labors American Mineralogy stands so deeply indebted.

San Francisco, Cal., Sept., 1864.

ART. VI.—*On the Crystallization of Brushite;* by JAMES D. DANA.

(Communicated to the California Academy of Sciences.)

THE crystals of the new mineral Brushite which I have had under examination were received from Mr. G. E. Moore, to whose chemical investigation science owes the first determination of the species.

The crystals are slender prisms, not over a third of an inch in length. A common form (containing all the occurring planes) is shown in the annexed figure. The prisms are monoclinic, and are often flattened parallel to the clinodiagonal, as here represented.

Cleavage is perfect parallel to the clinodiagonal section, or the plane $i\bar{i}$; also distinct, parallel to the lines cl, as apparent often in the cross fractures of crystals, and by occasional striæ. This plane of cleavage may be called the basal, or O.

The planes I and 1 are brilliant, especially the former. The oblique plane, situated on the back side in the figure, which may be called r, is quite rough, owing to an oscillatory combination of two hemi-octahedral planes. In many of the crystals, only the right one of the two planes I is present, and also only the left one of the two planes 1. The prisms frequently terminate above in an irregular edge made by the meeting of the one or two planes I and the rough plane r, and this edge is sometimes cut off, more or less deeply, by a single oblique plane, which is one of the planes 1.

According to measurements with the reflective goniometer—

$I : I$	$= 142° 26'$	$1 : i\bar{i}$	$= 101° 40'$
$I : i\bar{i}$	$= 108\ 47$	$1 : 1$	$= 156\ 20$ (approximately.)

The inclination of 1 on 1 could not be accurately measured on account of the minuteness of the planes in the crystals in which both planes occur, and the want of perfection in the reflection. The angle obtained for $1 : i\bar{i}$ would give for 1:1 156° 40′.

By measurement with a goniometer attached to a compound microscope, the plane angle between the lines of cross cleavage, or cl, and the edge $I : I$ (which equals the inclination of O on the orthodiagonal section or the plane $i\bar{i}$) was found to be 117°–117½°; and that between edge $I : I$ and edge 1 : 1 (which equals $i\bar{i}$ on $1\bar{i}$, both unobserved planes), 95° to 95½°; whence, $O : 1\bar{i}$ would equal, approximately, 147° 30′. The inclination of the rough plane r on the edge 1 : 1 is about 110°, but varies much.

The results of calculation, taking as data the above-mentioned

angles $I : I$ and $1 : i\bar{\imath}$, along with the inclination of O to $i\bar{\imath} = 117° 15'$, and that of the edge $1 : 1$ (or $1\bar{\imath}$) to $i\bar{\imath} = 95° 15'$, are as follows:

$$C\ (= O : i\bar{\imath}) = 117° 15' \text{ and } 62° 45'$$

$$a \text{ (vertical axis)} : b \text{ (clinodiagonal)} : c = 0{\cdot}5396 : 1 : 2{\cdot}614$$

$$1 : 1 = 156° 46' \qquad -1 : -1 \text{ (unobserved planes)} = 164° 22'$$

The species is related in form to Vivianite, in which

$$a : b : c = 1{\cdot}002 : 1 : 1{\cdot}3843;$$

for, if we double the a of Brushite, and halve the c, we have for the ratio of its axes—

$$2a : b : \tfrac{1}{2}c = 1{\cdot}0792 : 1 : 1{\cdot}307.$$

The two species are also alike in the perfect and pearly clinodiagonal cleavage.

ART. VII.—*Introduction to the Mathematical Principles of the Nebular Theory, or Planetology;* by GUSTAVUS HINRICHS, Professor of Physics and Chemistry, Iowa State University.

THE *nebular hypothesis*—the boldest thought that ever elevated the human mind, by bringing us, as it were, in sight of the mysterious fiat of the Almighty—was, in its great general features, unfolded almost at the same time by Germany's deepest thinker, the Königsberg philosopher, IMMANUEL KANT, and by PIERRE SIMON DE LAPLACE, the greatest mathematician of France. It is truly the closing stone in the philosophy of the celestial vault; for Copernicus and Kepler made us behold the foundation,—the first, by placing the sun as the lantern of the world in the center, and surrounding it with the planets—the second, by destroying the cycles and unravelling the harmony of the spheres in his immortal laws; and after the existing phenomena had thus been rightly viewed, Newton made us behold the invisible bond that connects the members of the system, while at length Kant and Laplace pointed out to us the hand that at "the beginning" projected these celestial balls into space and thereby insured the continued existence of the system.

But notwithstanding this noble parentage and its being the *logical sequence* of the discoveries in the theory of cosmos made by Copernicus, Kepler and Newton, the nebular theory enjoys as yet but slight consideration among astronomers. Arago[1] is the only one of these who has deigned to consider it earnestly, and he probably did so more in his capacity as a physicist than as an astronomer.

[1] Arago, Astronomie Populaire, ii, 7. Paris and Leipsic, 1855.

The reason of this neglect seems to be the incomplete state in which even Laplace himself left the theory. Direct observation, moreover, seemed to contradict some laws given as necessary consequences of this hypothesis.

We have already, in a former article,[2] tried to vindicate the theory in this last respect by showing that the hypothesis is really confirmed even in these apparently contradicting observations. We will now endeavor to give a somewhat more complete development to the fundamental principles of Kant and Laplace, and to exhibit the exact position of the nebular theory itself, hoping thereby to show that this theory, if we only study it earnestly and patiently both by experiment and analysis, fully deserves our confidence.

As this subject is as vast as it is difficult, we beg the critic always to keep in mind that we do not pretend to give a treatise, but merely offer an *introduction* to this almost new field of analysis.

We commence with a short survey of the fundamental principles and the aim of the theory of the solar system, in order clearly to understand why the nebular theory is necessary, what it will have to accomplish, and how far it already has done its duty.

§ 1. *The fundamental constants of the Solar system.*

As the discovery of a law of nature is but the reduction of the infinitude of observed quantities to a few constants by means of a function, the algebraic expression of the law—we see that *the progress of astronomy to a great extent must be identical with the reduction of the number of such constants.* This is fully borne out by the history of the science. For, while the Ptolemaic theory[3] of the planetary motion required the radii and inclinations of seventeen different circles to express the observed motions of Saturn, Kepler reduced this number of constants to three, the semi-major axis, eccentricity, and inclination of the orbit. This very principle is also placed by Laplace[4] at the head of his *Mécanique Céleste.*

We may therefore trust in this principle, and with Laplace try to reduce the number of indispensable constants. We must first, however, ascertain which are those constants that are now *considered* fundamental or indispensable.

The *constants of the solar system* now exclusively deduced from observation are:

1. The *mass*, m, of the planet.

[2] The density, rotation, and relative age of the planets: this Journal, Jan., 1864.

[3] Fracastor; see Bailly, Histoire de l'Astronomie moderne; vol. i. Paris, 1779. Eclaircissements, livre iv, § 23, and livre viii, § 27.

[4] Il importe extrêmement d'en bannir tout empirisme et de la réduire à n'emprunter de l'observation que les données indispensables.—*Méc. Cél.—Plan.*

2. The *figure*. On assuming a primitive fluidity and a small velocity of rotation, theory gives an ellipsoidal figure of the planets; hence not independent of observation.

3. The *ellipticity* of the planet. Theory may assign to it a higher and a lower limit by means of the mass (1) and the angular velocity (6)—but though the connection of this constant with others thereby is manifest, still its exact value can only be derived from observation.

4. The *volume*, or *diameter*, of a planet. Combining this constant with the first (mass) we obtain the *density*.

5. The *plane* (or *inclination* of the axis),

6. The *direction*, and

7. The *velocity of rotation*. Though this last element bears relation to others, still its exact value can only be obtained from observation. Even if Kirkwood's law of rotation[*] should prove to be perfectly exact, this element would continue to be a fundamental constant as long as that law remains an empirical one.

8. The *distance*, a, of a planet from the sun, or the semi-major axis of its orbit. By the theoretically proved third law of Kepler, we get from this constant the *periodic time*, T, requiring only the *constant*, μ, *of gravitation* to be known, and this latter is the same for all planets.

9. The *plane* or *inclination*, i, of the orbit.

10. The *direction* of the motion. The *velocity* is given by the distance.

11. The *eccentricity of the orbit*. This constant *is* fundamental, for the theory of gravitation only proves the orbit to be a *conic section* of some kind. The eccentricity can only be found from observation.

12. The *number of satellites* of a planet is also fundamental—and for each of them the same eleven constants have to be taken from observation; the first seven even are required by the sun.

From these twelve empirical data, theoretical astronomy can deduce the motion of the corresponding planet. The whole number of empirical constants is not at all inconsiderable; for

8 principal planets, constants	1–12,	- - -	96
80 small planets, "	8–11,	- - -	320
23 secondary planets, "	1–11,	- - -	253
The sun, "	1–7,	- - -	7
Total number of constants,			676

to which the corresponding constants for the comets would have to be added.

What, in the face of this great number of constants that astronomy has to borrow from observation, shall we say about the boasted perfection of this science? Is it not in science, as in

[*] This Journal, ix, 395, May, 1850; also xiv, 210, Sept., 1852.

morals, that self-adoration hinders progress? Can any astronomer who has not merely studied the details of the celestial mechanics, but also kept in mind the great principle laid down by its author in his "Plan"—can he still pretend that Newton's theory of the solar system merely needs further development, seeing that the few bodies of this system require him to borrow about seven hundred constants from observation?

We shall honor the memory of Newton much more by trying to go beyond the results of his labors than by stupidly worshipping[6] the same, and thus arresting the progress of that science to promote which he spent his life.

§ 2. *These fundamental constants sustain remarkable relations to each other.*

The Newtonian theory of gravitation simply accepts these constants as observation gives them. For if our earth had Jupiter's mass, the rings of Saturn, the moons of Uranus and its axis in the ecliptic, the latter perpendicular to the orbit of Jupiter, a retrograde motion in a hyperbolic orbit—it still would as fully and as beautifully confirm the theory of universal gravitation as it does now; for, let us openly and frankly acknowledge it, these constants are independent of the theory of gravitation because the latter is independent of the former.

But, though this theory does not give any reasons for any kind of dependence between the often mentioned constants, observation shows that they sustain very remarkable relations to each other;[7] or in other words, *there are relations and laws in our solar*

[6] The literature of astronomy teems with implicit instances hereof; but we find also direct expressions of this feeling, like the following:

Enfin, nous avons vu que ces resultats eux mêmes peuvent se composer en un seul et se representer par une loi unique, celle de la Pésanteur universelle; parvenus à principe nous nous voyons en quelque sorte élévé à *la source commune de tous les faits astronomiques; tous en dérivent* de la manière la plus simple et ils y sont en quelque sorte comme concentrés. Nous avons donc pour ainsi dire décomposé le systéme du monde, *nous l'avons réduit à son élément unique*, et nous l'avons ensuite recomposé.—Biot, *Traité élém. d'astronomie physique.* Paris, 1805. Concluding remark of the work.

This "élément unique" is rather singularly unique, requiring no less than seven hundred elements to be borrowed from observation alone!

[7] Newton was aware of this—indeed, nobody can help seeing some of these relations. In the scholium to the third book of Principia he says:

"Planetæ sex principalis revolvuntur circum solem in circulis soli concentricis, eâdem motus directione, in eodem plano quamproximé. Lunæ decem revolvuntur circum terram, jovem et saturnam in circulis concentricis, eâdem motus directione, in planis orbium planetarum quamproximæ. *Et hi omnes motus regulares originem non habent ex causis mechanicis.*" Edit. *le Seur* et *Jacquier* Geneva 1749–42.

It is customary to censure Kepler's fancy in contrast to the solidity of all Newton's words; still a sentence like the above is much more objectionable in science than the boldest fancy, for the latter is not accepted without severe scrutiny, while the former is repeated as a sacred truth. If Newton had written "mechanical causes known to me" instead of by "mechanical causes," simply to imply that he knew them all, he would have prevented many a drawback that has encumbered

world of a still higher order than those deduced from gravitation. Thus, the inclinations of the orbits of the principal planets, instead of being uniformly distributed over the first quadrant, are all very small; and their direction, instead of being as often retrograde as direct, are for *all* planets and most satellites *direct.* Instead of having the eccentricities regularly varying from 0 to ∞, we find them for all planets nearly zero! The same may be said of any of the above fundamental constants, and not least of the distance, as it is found *approximating* to Titius–Bode's law, that is, to

$$a_n = 4 + 3 \cdot 2^{n-1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

But quantities that sustain mutual relations to each other are the particular values of a certain function for definite given values of the variable quantities; hence, if we intend to be true to the spirit expressed by the words of Laplace above quoted, it is a problem legitimately belonging to astronomy *to find these functions of which the fundamental constants are but particular values.*

Let us boldly face this great problem and not desist though astronomers tell us that it is not part of their science.

§ 3. *The fundamental constants satisfy the conditions of stability of the system.*

Since relations exist between the values of the fundamental constants, we may ask for the most general expression of these relations. The principal of these relations are, by Lagrange and Laplace, proved to be such as to fulfill the conditions of stability of the system. These conditions are—

1. *Incommensurability of the times of rotation,* ensured by the distances forming an exponential series (1).
2. The *central mass* vastly preponderating, and the greatest masses revolving where the mutual distances are the most considerable.
3. The *direction of all motions* is the same.
4. *The plane of all orbits is and remains nearly the same,* because

$$\Sigma m \sqrt{a} \cdot tg^2 i = c_1, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

is and remains a small quantity (the letters having the same signification as in § 1).

science. Less arrogant, because more true, he is when writing to Burnett (about 1680–81), "but yet I must confess I know no sufficient cause of y^e^ earth's diurnal motion."

Even Biot, notwithstanding his blind admiration, above cited, cannot help seeing something *more than* gravitation can account for, when noticing the harmony in the rotary and translatory motions. He says: "cet accord, *qui tient sans doute aux premières causes qui ont déterminé les mouvements planétaires, est un des phénomènes les plus remarquables du système du monde.*—Astron. physique, vol. iv, chap. v, p. 467.

5. *The eccentricity, e, of the orbit is and remains nearly the same,* because

$$\Sigma m \sqrt{a} . e^2 = c_2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

is and remains a small quantity.

6. The *density, d,* is such that the diameter of the bodies is small in comparison to their distances.

We may add—

7. The *form* of the planets is such that the influence of the deviation from a sphere is the smallest possible.

§ 4. *Gravitation is insufficient.*

The laws of Kepler are grand—as well as Newton's theory in accounting for them; but the above laws of Lagrange and Laplace are certainly of a superior order, and the theory of gravitation in failing to give even a shadow of a reason for these laws proves itself to be not the whole truth: we must go beyond this force!

Astronomers seem to forget the *history* of their own science; for how could they otherwise deny the legitimacy of accounting for the fact that the above laws express the stability of the world? Had not astronomers at the time of Kepler the same reason to be satisfied with his laws as astronomers have now in abiding by the laws of stability? And is it not as urgent to discover the causal connection between these laws ensuring great *duration* to our system, as it was to find in gravitation the mechanical cause of those laws ruling the spheres at the time being?

Now the hypothesis of Kant and Laplace will be found to account for the laws of stability as rigidly as the hypothesis of Newton accounts for the laws of Kepler; why, then, deride the former and adore the latter hypothesis? Or do we even forget that "the principle of gravitation" is but a "*hypothesis?*" Did not Newton himself consider it as such? Is not this force fully as mysterious and fully as much beyond the reach of direct observation as the chaos of Kant and Laplace? The former we *assume* as continually acting, because we find that *the motions* are such as this force would produce (*provided* a tangential force, of which gravitation knows nothing, also acts in a definite manner, etc., etc.). Why not also assume the latter as having been real, if we find by the same mechanical deductions that the existing harmony, as expressed in the stability of the system and ensured in the mutual relations of the fundamental constants follows directly from the above-named chaos? If in the one case we reason from fact or law to cause—why not also in the other, provided our conclusion is as legitimate?

Here astronomers will not fail to object that this last condition

is *not* satisfied. We fully admit this; but beg them to remember that it has taken two centuries of labor to ensure this legitimacy to gravitation—that Newton did not leave gravitation as a mere suggestion (as such it had existence before him), but in his immortal *Principia* gave the necessary mechanical firmness to this hypothesis: how different has been the lot of Kant and Laplace's hypothesis! The first of these expounded it rather fancifully in connection with speculations on the inhabitants of distant globes;[8] the latter only gave a few bold and deep outlines of the nucleus of the theory! What would to-day be the estimation of gravitation if we, instead of Newton's *Principia*, only had a few of Hooke's sublime guesses, if these had only been considered by men like Fontenelle[9] instead of being investigated by Euler, the Bernoullis, Lagrange, Laplace, Gauss, Hansen, Plana, etc.?

Can anything be more unjust than exaltation of the hypothesis of Newton—this deservedly cherished subject of the master minds of two centuries—above the hypothesis of Kant–Laplace, which, being too early left even by its astronomical parent, has been ever since considered an outcast in the world, endangering the reputation of any one who would dare to touch it?

We will adopt this almost forlorn hypothesis as a *mere hypothesis*—we will patiently and carefully trace its bearings by means of as rigid an analysis as we can command in this most intricate field; we will minutely compare the results thus obtained with the actually observed state of things; and if we find *the correspondence between idea and phenomenon*, between analysis and observation, to be very close, we hope that those who have analysis more at their command than we, will pay as much attention to this high branch of astronomy as has been, and deservedly continues to be, bestowed on Newton's hypothesis of gravitation. If our feeble endeavors only succeed in making Kant–Laplace's *hypothesis* admitted as such among analysts, we shall have accomplished all we desire: for then this hypothesis will soon be considered as firmly established a principle as gravitation,[10] or as the fact of the rotation of the earth,[11] which latter—notwithstanding Foucault's pendulum and gyroscopes—still remains unproved by *ocular evidence*, all "demonstrations" being in fact

[8] Allgemeine Naturgeschichte und Theorie des Himmels, 1755.

[9] Théorie des Tourbillons Cartésiens avec des réflections sur l'attraction (1752). Also his Entretiens sur la pluralité des mondes.

[10] Gravitation was always treated as a mere hypothesis by the Cartesians; the work of Fontenelle above cited offers the instance most generally known.

[11] The scientific prejudice existing against the nebular theory is perhaps as injurious as the religious prejudice once "*resisting*" the motion of the earth. See the amusing statement in the preface to the 'Dei massimi sistemi,' (ed. Padova, 1744,) that the "'Moto della Terra' non può né dee ammettersi se non come pura ipotesi matematica, che serve a spiegare più agevolmente certi fenomeni." Also Galileo himself, in his Dialogue (*Opere compl.* Firenze. Vol. i, 1848, pp. 387 and 447).

inductive, reasonings with our senses. Thus, in the latter, we *see* the plane of the oscillations rotate, but *conclude* it to be the earth. It requires at least as much mental effort to apprehend its true bearing as the simple reference to the diurnal motion of sun, moon, and stars.

§ 5. *How far the nebular theory accounts for the stability of the system.*

The theory of gravitation can never, therefore, account for the stability of the solar system. How far, then, does the nebular theory explain those great fundamental conditions of the system that ensure not only the harmony of the solar world but even make this harmony (almost) permanent?

In order to invite physicists and astronomers to the perusal of the following introduction, we will try to give a simple answer to this most important question.

I. *The plane of all planetary orbits must be nearly the same* (see § 1, 9, and § 3, 4, also § 10).

This theorem has been clearly seen by Kant and Laplace; it is the most immediate expression of the hypothesis. Mr. Trowbridge has recently pointed out[13] to us a very interesting consequence hereof, viz: the most distant planet *must move in the invariable plane;* and, indeed, the inclination of the orbit of Neptune is 1° 47′, that of the invariable plane 1° 41′.

II. *The direction of all planetary revolutions must be the same;* this obvious consequence of the hypothesis accounts for § 3, 3, and § 1, 10.

III. *The eccentricity of the planetary orbits must be very small*— accounting for observation, § 1, 11, and condition of stability, § 3, 5 and § 10.

This proposition has been deduced in general reasonings by Kant and Laplace; in the following we will try to give a demonstration of it.

IV. *The planetary distances are such that the successive planets were evolved at equal intervals of time;* or if $t=1, 2, 3, \ldots$. respectively for Mercury, Venus, Earth ...., then the distance is

$$a_t = \alpha + \beta \cdot \gamma^t, \quad \ldots\ldots \quad (4)$$

where α, β, γ are constants, and *t the age of the planets* above that of Mercury. From this follows the condition of stability that *the periodic times are incommensurable* (§ 3, 1). Besides, it is seen that this law accounts for the empirical law of Bode (§ 1, 8).

The analytical demonstration of this law is one of the principal objects attempted in the present introduction. (See § 13.)

V. *The mass of the more distant planets is the greater,* on account of the greater space from which the material of the planet was condensed (space increasing according to IV). This is confirmed

[13] On the Nebular Hypothesis, § 24; this Journal, November, 1864, page 355.

by the fact that the sum of the *four great exterior planets* is 480 times the mass of the earth, while the sum of the masses of the *four interior planets* is but twice the mass of the earth. Furthermore, *the mass must increase toward the sun*—as the density from which the rings were formed was greater nearer the center. This is confirmed, as the mass of Neptune is 25·6, the mass of Uranus but 14·5, so that the mean of the two most distant is 20; the mass of Saturn is *five* times as great (101·6), and the mass of Jupiter, again, three times greater (339·2). The minimum in the case of the mass of Uranus is evidently produced by the simultaneous influence of both the above principles.

The mass of Jupiter is a maximum; it is so great that *the next following ring was broken up into fragments by the perturbing influence of so stupendous a mass*—thus originating the host of *asteroids*, and perhaps also the *meteorites*.

The very small mass of the interior planets as compared with the exterior ones is not astonishing, if we remember that the inter-planetary space between Jupiter and Saturn is to that between Venus and the Earth, as 10^3–$5{\cdot}2^3$ to $1{\cdot}00^3$–$\cdot72^3$, or as 860 to ·63, or nearly as 1300 to 1. The mass of Jupiter is to the mass of our earth as 340 : 1, thus giving us still some margin for an increase in density toward the center of the nebula.

Being as yet unable to give the precise *theoretical law of the masses*, we are obliged to make the above few suggestions, in order to show that the nebular hypothesis at least gives a general law of distribution of the planetary masses in conformity with § 1, 1, and § 3, 2. (Prof. Kirkwood takes a similar view of the asteroids; see this Journal, 1852, xiv, 214.)

VI. *The figure of the planets* (being a *condensed* vapor) *must be an oblate spheroid* of

VII. *Small ellipticity*, because

VIII. *The velocity of rotation is but small* (compare § 1, 2, 3, 7, and § 3, 7).

This last proposition is based upon the fact that the moment of rotation is but the *difference* between the moment of revolution of the exterior and interior part of the planetary ring.

Still, *the exact amount of this velocity*, as well as the period of rotation of the sun, has not yet been deduced from the nebular hypothesis; we have often attempted it, but as yet have not been able to solve this difficult problem.

Prof. Kirkwood has found[13] the empirical law of the velocity of rotation, *a law analogous to the third law of Kepler*. We have repeatedly arrived at expressions similar to (but not identical with) Kirkwood's law.

IX. *The Plane;* and

[13] As above, this Journal, 1850, ix, 395.

X. *The direction of rotation* of the planets has been considered at variance with the nebular theory, ever since the discovery of the lunar system of Uranus. We believe that our analysis of this problem[14] shows that the rotation of Uranus and Neptune, both as to position of the axis and direction of motion affords a very interesting confirmation of the theory. See § 1, 5, 6.

XI. *The density* of the planets has also been considered as being adverse to the theory; but if, as necessary, the influence of the age is taken into account, it is found that the minimum density exhibited by Saturn is demanded by the theory.[15] Compare § 1, 4, and § 3, 6.

XII. *The number of satellites* was already shown, by Kant, to increase with the distance from the center of the nebula. Though not usually given as such, it nevertheless is a condition of stability of the system—at any rate it is conformable to observation (§ 1, 12).

The rings of Saturn are best considered as a host of satelloids, corresponding to the planetoids (and meteorites) of the solar world—thereby accounting for the excessive thinness and the subdivisions of the rings.[16]

In looking back upon the preceding account of the present aspect of the nebular theory, it will be seen

A. That the four great fundamental conditions of stability referring to *the system at large* are now satisfactorily deduced from the hypothesis of Kant–Laplace (I–IV above).

B. That the problem of the *mass* (V) and the *number of satellites* (XII), though not completely evolved, still is sufficiently comprehended to enable us to say that the analytical solution is *possible;* and

C. That the elements referring to the *single planets*, or rather their subordinate systems, are, with the exception of the *exact* law of rotation (VIII), fully deducible from the fundamental hypothesis of Kant and Laplace.

We see, then, that the *fundamental constants* of the solar system, which number about seven hundred (§ 1), exhibit very remarkable mutual dependencies (§ 2), which are such as ensure the permanence or stability of the system (§ 3), which Newton's law of gravitation cannot account for (§ 4). Though they offer a higher problem for theory than Kepler's laws, astronomers have hitherto been unwilling to recognize the analysis of the above conditions of stability as part of their science. Laplace, while instrumental in bringing to light the great laws of the stability of the system, independently reproduced the bold hy-

[14] On the density, rotation, and relative age of the planets; this Journal, 1864, xxxvii, 36, 48.

[15] As above, p. 49.

[16] On the density, etc.; this Journal, 1864, xxxvii, 54.

pothesis of Kant, and though this has been most grievously neglected by analysts and astronomers, still it now affords us a full solution of the four great harmonies ensuring the permanency of the solar world, and also solves most, and at least indicates the solution of, all other problems relating to the harmony of the fundamental constants of the solar system.

May we not hope that astronomers will begin to bestow on this theory some share of their labor?

§ 6. *The Hypothesis.*

We assume, with Kant and Laplace, as the primitive condition of the solar system, or as *nebula:*

The space of the solar system was filled with matter having a moment of rotation.

This *matter* is endowed with the same forces we know it to possess; a simple calculation will furthermore show that it was a highly rare vapor. Its chemical constitution we will leave out of consideration for the present; we therefore consider it as composed of the elements we know here on earth, many of which we *now* know to be found on the sun, and are probably also on the distant stars;[17] still there can be no doubt but that many more elements exist than we are acquainted with. Many of the spectral lines even of our own central star are irreducible to spectra of known elements. We therefore mean simply to say that at the above primitive period the *elements had been created.* I hope at some future time to publish an attempt at a *mechanical theory of the elementary bodies*, which has occupied my time for about ten years, and wherein I endeavor to show the physical properties of the known elements to be *definite functions* of their *atomic number and form.* Accordingly, there would yet remain a more primitive condition, the existence of the *one primitive matter* (Urstoff) which would be considered as the direct creation of the *one* God.

The *rotation* of the nebula is *not* to be thought regular, but simply amounting to a certain momentum. I have elsewhere[18] tried to show that such rotation may be considered as the *effect of a difference of any kind between the primitive forces of attraction and repulsion* wherewith we know matter to have been endowed.

If, therefore, these views should be well founded, we should have arrived at the grandest principle we can conceive of in the present state of our knowledge; we should be able to see how from *created matter alone the whole of the solar system has been developed;* we would be enabled to conceive the almighty *fiat* as

[17] Rutherford, Astronomical Observations with the Spectroscope; this Journal, 1863, xxxv, 71. Above all, Bunsen's and Kirchhoff's memoirs on their great discovery.

[18] This Journal, 1864, xxxvii, 52.

one single act. How much such a theory would tend to elevate our conceptions of the great Author, we cannot here develop.

In the present paper I shall not go farther back in time than to the existence of the nebula of Kant and Laplace as above defined.[19]

§ 7. *Plateau's experiment.*

Before entering upon the analysis of the nebula, we must refer to the experimental evidence of the nebular theory afforded by the beautiful experiments of Plateau, detailed in his *Mémoire sur les phénomènes que présente une masse liquide libre et soustraite à l'action de la pésanteur*, Pt. I (Neuv. mém. de l'Acad. de Bruxelles, vol. xvi, 1843). His results are:

1. A liquid, subject only to the action of its molecular forces assumes the form of a perfect sphere (§ 2).

2. This globe is flattened at its poles, if subject to rotation (§ 10). Although, as he thinks, the molecular forces are not *identical* with those acting in the nebula, still the results ought to be analogous, if they are not identical.

[19] It is perhaps not out of place here to give a synopsis of the different distinct *ages* that are characterized by a further individualization or a new direct creation according to the views above indicated.

Three (or four) direct acts of the Deity may be recognized, viz: the creation of *matter*, of *life*, of *mind* (and the *redemption*). The formation of the *elements* out of matter characterizes the *first age;* the formation of the *solar world*, with its planets, moons and central sun, the *second age;* while the *third age* beheld the development of our *earth* from a vaporous ball to its present shape; in the *fourth age, life* was created in the form of *plants* and *animals;* in the *fifth age*, mind, the investigating mind, was introduced by the creation of *man*, the *cephalized animal;* and in the *sixth*, redemption took place; while a *seventh age* will behold the destruction of the whole system, occasioned by the *extinction* of the solar body and the *resistance* of ether. (See this Journal, 1864, xxxvii, 56)

To *every age* correspond two sciences: the first relates to the development—Whewell would call it the *Paletiology* of the age—while the second relates to the actually *existing product* of the development or creation of that age, i. e., *the* science. Thus we obtain the following general view of the natural sciences:

Age	1	2	3	4	5	(6)	(7)
	I. *Creation of* MATTER. Development of			I. Creation of	III. Creation of	*Redemption.*	Destruction of the world.
	Elements.	*Solar system.*	*Earth.*	*Life.*	*Mind.*		
Paletiology	Atomology	Planetology	Geology	Paleontology.	Archeology.	Old Testament	
Science.	Physics, Chemistry.	Astronomy.	Geography.	Botany, Zoology.	History.	New Testament	

These names have of course to be taken in their widest sense; thus geography stands for physical geography, meteorology, etc., and history comprehends not only political but also the intellectual history of the human race, thus including again *all* the sciences in their historical development. We see how "planetology" is allied to geology and astronomy.

3. If the rotation becomes sufficiently rapid, the *whole globe* is transformed into a *ring* in the equatorial plane (§§ 11–14) which, by continuing the rapid rotation of the large central disc, even loses its regularity, and separates into small masses which immediately assume globular forms. "But," continues he (§ 19), "this is not all; one or more of these spheres are always seen to take, at the instant of formation, a motion of rotation on their own axes, and this motion is almost always in the same direction as that of the ring."[20] He even found that still smaller globes were formed.

4. If a small disc is put into very rapid rotation, a ring is formed, while a part of the original globe remains on the axis (§ 21), so that Plateau rightly concludes with saying (§ 27) that most of the phenomena of the relative configuration of the heavenly bodies have been reproduced by him on a small scale.[21]

[*To be continued.*]

ART. VIII.—*Contributions to Chemistry from the Laboratory of the Lawrence Scientific School;* by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.—No. 2.

§ 1.

On the separation of chromium from aluminum, iron, manganese, cobalt, nickel, zinc and magnesium.—Sesquioxyd of chromium in an alkaline solution is readily oxydized to chromic acid by means of chlorine, bromine or deutoxyd of lead. When chlorine or bromine are employed as oxydizing agents, the alkaline solution may be neutralized by acetic acid, after the oxydation is complete, and the chromic acid may then be precipitated by acetate of barium, when the solution is free from sulphuric acid, and directly weighed in the form of chromate of barium. In place of free alkali it will be found in practice very much more convenient to employ acetate of sodium or potassium. When a solution of sesquioxyd of chromium is rendered nearly neutral by a solution of carbonate of sodium, and acetate of sodium is added in excess, a current of chlorine gas, or a solution of chlorine water, readily converts the whole of the chromium present into chromic acid, especially when the solution is hot, and when it is kept nearly neutral by occasional addition of carbonate of sodium. The excess of chlorine is easily expelled by boiling,

[20] Of course; for in this oil the density is the same. See our article, this Journal, 1864, xxxvii, 51, $\delta=0$.

[21] Malgré la difference des lois que suivent les forces attractives dans ce cas et celui des grandes masses planétaires, nous avons vu se produire, en petit, une représentation frappante de la pluspart des phénomènes de configuration relatifs aux corps célestes.

after which, in the presence of bases not precipitated by ammonia, the chromic acid may be precipitated by acetate of lead, or acetate of barium, and weighed in the form of chromate, provided, of course, that no sulphuric acid is present. When sulphate and chromate of barium are thrown down together, the chromic acid may be reduced to sesquioxyd by boiling with concentrated chlorhydric acid and alcohol, after which the barium may be precipitated by sulphuric acid, and the sesquioxyd of chromium thrown down in the filtrate by boiling with ammonia in the usual manner. As the reduction of chromate of barium by means of chlorhydric acid and alcohol does not take place very readily, it is better to boil the chromate with an excess of carbonate of potassium or sodium, to filter off the carbonate of barium, and determine the chromic acid by means of nitrate of suboxyd of mercury, or by reduction to oxyd of chromium and precipitation with ammonia in the usual manner.

When aluminum and iron are to be separated from chromium by this process, the two oxyds may be precipitated together by simply boiling, the solution after the complete oxydation of the chromium to chromic acid, in the presence of excess of acetate of sodium. It is more convenient and equally accurate to neutralize the solution with ammonia, separate the alumina and sesquioxyd of iron by filtration, and determine the chromium in the filtrate by reduction and precipitation with ammonia.

When the oxyds of calcium, magnesium, zinc, nickel, cobalt and manganese are present in a solution containing sesquioxyd of chromium, it is best to oxydize the chromium to chromic acid as above, and then to precipitate with acetate of lead or barium.

I have stated in a former paper that chromic iron ore may be completely resolved by fusion with fluohydrate of fluorid of potassium. In this and in all similar applications of the fluohydrate, it is best to evaporate the finely pulverized mineral to dryness with a concentrated solution of the salt. On subsequently heating to low redness, the resolution of the mineral is effected with the utmost ease, a portion of the chromium being usually oxydized to chromic acid by the oxygen of the air. After expelling the fluorine by heating the fused mass with sulphuric acid, the remaining mass may be treated with acetate of sodium and chlorine in the manner already pointed out, the iron and aluminum separated by boiling, and the chromic and sulphuric acids precipitated by acetate of barium, after which the chromium may be determined as above.

In precipitating chromic acid by means of nitrate of suboxyd of mercury, hot solutions must not be employed, as a small portion of chromic acid is always reduced to sesquioxyd of chromium. The precipitated chromate should be allowed to

stand some hours before filtering. In general, the precipitation by acetate of lead or acetate of barium is to be preferred even when the resulting chromate is to be weighed as such.

§ 2.

On the employment of acetate of sodium for the separation of iron and aluminum from other bases.—The facility with which iron and aluminum are precipitated from neutral solutions of the sesquioxyd, by boiling with acetate of potassium or sodium, has led to frequent analytical applications, though the method is not so generally employed as it deserves. Mr. C. F. Atkinson has devoted much time to a careful study of the subject, and has arrived at the following results, which appear to me worthy of attention. The sesquioxyds of iron and aluminum may be perfectly separated from the protoxyds of manganese, cobalt, nickel, zinc, magnesium and calcium, and from sesquioxyd of uranium, by boiling the neutral or nearly neutral solutions with acetate of sodium, provided that the following precautions are observed. The solutions from which the sesquioxyds are to be precipitated must be dilute: half a liter of the solution should not contain more than one gram of either sesquioxyd or of the two when both are present. The quantity of acetate of sodium should be sufficient to convert by double decomposition all the bases present into neutral acetates. The acetate should be added to the metallic solution when cold and the whole should then be heated together and boiled for a short time. It is not necessary to filter upon a water-bath funnel, but the beaker containing the solution should be kept nearly at the boiling point during filtration, and a ribbed filter should be employed. In all cases it is best to add a few drops of free acetic acid to the solution, to prevent the formation of basic acetates of the protoxyds. This is especially necessary in separating iron and aluminum from zinc and nickel.

Finally, it is best, whenever it is possible, to have all the bases present in the form of chlorids. The iron and alumina upon the filter in the form of basic acetates must, whenever an absolutely complete separation is necessary, be redissolved in chlorhydric acid and again precipitated by boiling with the acetate after rendering the solutions nearly neutral by means of carbonate of sodium. In this manner only is it possible to separate the last traces of the stronger bases. Finally, the basic salts of the iron and aluminum, after washing, must be redissolved in chlorhydric acid, and precipitated by boiling with ammonia in the usual manner, to free them completely from alkali. The precaution of a second treatment with acetate of sodium is more necessary with alumina than with sesquioxyd of iron alone. It is scarcely worth the trouble in the separation of iron from calcium and magnesium.

According to my own observations, the sesquioxyds of iron and aluminum cannot be separated from sesquioxyd of chromium by boiling with acetate of sodium, although the last mentioned oxyd is not precipitated when alone in solution. In this case it is necessary to oxydize the chromium to chromic acid by chlorine in the manner already pointed out.

§ 3.

On the separation of manganese from cobalt, nickel and zinc.— Schiel's method of separating manganese from the alkaline earths by adding acetate of sodium to the mixed solutions, heating the liquid gently and then passing chlorine through it so as to convert the manganese into a hydrate of the sesquioxyd, is better than that formerly given by myself in which peroxyd of lead is used as the oxydizing agent. With respect to Schiel's method, however, it must be remarked that it cannot be employed to separate manganese from nickel or cobalt, because both of these metals are converted into higher oxyds under the same circumstances. Nickel may, as Popp has recently shown, be completely precipitated as a deep blue hyperoxyd, while as I have myself observed, cobalt is also oxydized, though not precipitated, unless the solution is boiled with an alkaline carbonate. In separating manganese from zinc, calcium or magnesium, I have repeatedly found that a second treatment is necessary in order to obtain a perfect separation. This second treatment may be neglected in separating manganese from calcium and magnesium, but not in separating it from zinc, although the addition of a few drops of free acetic acid renders the process more exact.

Though the method of separating manganese from other bases by means of peroxyd of lead, which I formerly proposed, will hardly be used in future, now that we are in possession of more convenient processes, it will still be of some interest to chemists to know the precise nature of the insoluble black compound which is formed when peroxyd of lead, PbO_2, is digested or boiled with an excess of a solution of chlorid or nitrate of manganese and afterward thoroughly washed. An analysis of this body, made some years since in my laboratory by my lamented friend and former pupil, Mr. Theodore Parkman, gave the following results:

		Anhydrous.	Theory.
Manganese, - -	35·10	37·96	37·53
Lead, - - -	32·49	35·13	35·26
Oxygen, - - -	24·87	26·90	27·20
Water, - - -	7·52	....	....
	100·00	100·00	100·00

Neglecting the water, which may have been, in part at least, mechanically combined, and which amounts to between three

and four equivalents, we have the formula, MnO_2+4PbO_2, as the simplest expression of the results of the analysis.

A simple and perfectly satisfactory process for separating manganese from cobalt, nickel, and zinc, is the following: To the neutral or nearly neutral solution of the chlorids, acetate of sodium is to be added in excess together with a few drops of free acetic acid. The solution is then to be boiled, and a rapid current of sulphydric acid gas passed through it while boiling and continued for half an hour. Every trace of cobalt, nickel and zinc, is precipitated in the form of sulphid, while the whole of the manganese remains in solution. The precipitate is to be thrown upon a ribbed filter and quickly washed with cold water saturated with sulphydric acid gas. It is easily washed, and though the sulphids of cobalt and nickel precipitated in this manner are far more easily oxydized than when precipitated by boiling sulphid of sodium from boiling solutions, they will be found to present no difficulty as regards oxydation upon the filter. Manganese may then be determined in the filtrate by boiling with chlorhydric acid and precipitating in the usual manner with carbonate of sodium. The mixed sulphids upon the filter, supposing for the sake of generality that all three are present, are to be dissolved in chlorhydric acid and the metals converted into double cyanids by means of an excess of cyanid of potassium, after which the zinc may be precipitated by means of sulphid of sodium, as recommended by Wöhler.

When perfectly pure cyanid of potassium is not at hand, the following process will be found particularly convenient. Acetate of sodium is to be added to the solution of the mixed chlorids, after which the vapor of cyanhydric acid generated in a flask from sulphuric acid and ferrocyanid of potassium is to be passed directly into the solution. Cyanid of zinc is immediately precipitated more or less completely as a perfectly white powder. A solution of sulphid of sodium is then to be added as long as a precipitate is formed, after which the sulphid of zinc is to be separated by filtration. Cobalt and nickel remain in solution as double cyanids. The same process may be used to separate manganese from cobalt and nickel, sulphid of sodium throwing down under these circumstances a pure flesh red precipitate. It is easy to see that zinc and manganese together may be separated from cobalt and nickel by the same process and at one operation. No cyanid of manganese appears to be formed when cyanhydric acid is passed into a solution containing a salt of manganese, acetic acid, and acetate of sodium.

I have stated in a former paper[3] that the sulphids of nickel and cobalt are thrown down from boiling solutions by a boiling solution of sulphid of sodium in an insoluble form, so that in

[3] This Journal, March, 1864.

fact even strong chlorhydric acid scarcely exerts upon them an appreciable action. This process has been applied to the separation of cobalt and nickel from zinc and manganese by my excellent assistant, Mr. Maurice Perkins, and gives results which are very satisfactory, especially for qualitative purposes, the sulphids of manganese and zinc precipitated under the same circumstances being readily soluble, even in dilute acid. The process is now substituted in this laboratory for that given in most of the recent works on qualitative analysis, and has been repeatedly tested with satisfactory results.

§ 4.

On the separation of cobalt from nickel.—A method of separating these metals, given some years since by Liebig,[4] consists in boiling the mixed double cyanids of nickel and potassium and cobalt and potassium with oxyd of mercury. Oxyd of nickel is precipitated, while an equivalent quantity of mercury is dissolved as cyanid. The method certainly gives good results but is not free from objection. Long boiling is necessary before the precipitation is complete, and it is difficult to prevent "bumping" during ebullition. The excess of oxyd of mercury must be separated from the oxyd of nickel by a special operation, and the nickel afterwards again precipitated by caustic alkali.

These inconveniences may be completely avoided by employing, instead of the oxyd alone, a solution of the oxyd in the cyanid of mercury. When this solution is added to a hot solution of the double cyanid of nickel and potassium, the whole of the nickel is immediately thrown down as a pale green hydrate of the protoxyd. Under the same circumstances cobalt is not precipitated from the double cyanid of cobalt and potassium. Mr. W. N. Hill, who has repeatedly employed this method and carefully tested it, has found that the separation effected is complete. No cobalt can be detected in the precipitated oxyd of nickel by the blowpipe, nor can nickel be detected in the cobalt (finally separated as oxyd) by Plattner's process with the gold bead. The solution of oxyd of mercury is easily obtained by boiling the oxyd with a strong solution of the cyanid, and filtering. According to Kühn[5] the cyanid formed in this manner has the formula $HgCy+3HgO$. The hydrated oxyd of nickel precipitated may be filtered off, washed, dried, ignited and weighed. The cobalt is more readily and conveniently determined by difference, when, as is always possible, the two metals have been weighed together as sulphates. I am not prepared to say that this modification of Liebig's method of separating nickel and cobalt gives better results than Stromeyer's process by

[4] Ann. der Chemie und Pharmacie, lxv, 244.
[5] Berzelius, Lehrbuch der Chemie, iii, 872.

means of nitrite of potassium, but it is at least very much more convenient and requires much less time. The complete precipitation of cobalt in the form of $Co_2O_3,2NO_3+3KONO_3$ usually requires at least forty-eight hours, and rarely succeeds perfectly except in experienced hands.

§ 5.

On the separation of uranium from zinc, cobalt and nickel.—The method which I have given above for the separation of manganese from cobalt, zinc, and nickel, by precipitating the sulphids of the three last named metals, by means of sulphydric acid gas, from a boiling solution of the acetates, may be also used, according to the carefully conducted experiments of Mr. Perkins, for the separation of uranium from the same metals. The process is in all respects the same, and requires, therefore, no further description. It will be found much simpler and more convenient than that described by Rose, by means of carbonate of barium.

§ 6.

On the electrolytic precipitation of copper and nickel as a method of analysis.—The precipitation of copper by zinc, in a platinum vessel, with the precautions recommended by Fresenius, leaves nothing to be desired, so far as accuracy, ease and rapidity of execution are concerned. The method labors, however, under a single disadvantage: the introduction of zinc renders it difficult, or at least inconvenient, to determine with accuracy other elements which may be present with the copper. It has occurred to me that this difficulty might be overcome, the principle of the method being still retained, by precipitating the copper by electrolysis with a separate rheomotor. The following numerical results, which are due to Mr. E. V. M'Candless, will satisfactorily show the advantages of the method for the particular cases in which it is desirable to employ it. The copper was, in each case, in the form of sulphate; the deposition took place in a small platinum capsule, which was made to form the negative electrode of a Bunsen's battery of one or two cells, in rather feeble action. The positive electrode consisted of a stout platinum wire, plunged into the surface of the solution of copper at its center. The following table gives the results obtained in the analysis of pure sulphate of copper:

Number.	Salt taken.	Copper found.	Percentage.
I.	1·2375	0·3145	25·41
II.	0·4235	0·1075	25·38
III.	1·0640	0·2705	25·42
IV.	1·3580	0·3440	25·33
V.	0·5665	0·1450	25·59
VI.	0·4735	0·1205	25·48

In seven determinations of copper in the alloy of copper and nickel employed by the government for small coins, the following results were obtained:

Number.	Weight of alloy.	Copper.	Percentage.
I.	0·4160	0·3640	87·50
II.	0·6180	0·5410	87·54
III.	0·4600	0·4090	88·91
IV.	0·5120	0·4481	87·51
V.	0·4220	0·3693	87·51
VI.	0·2525	0·2225	88·11
VII.	0·3705	0·3255	87·85

The percentage of copper required by the formula $CuO, SO_3 + 5HO$ is 25·42, while the government standard alloy of nickel and copper contains 87·50 per cent of copper. The time required for precipitation varied from one to three hours, the separation of the last traces of copper being in each case determined by testing a drop of the liquid upon a porcelain plate with sulphuretted hydrogen water. The copper, after precipitation, was washed with distilled water, dried in vacuo over sulphuric acid, and weighed with the platinum vessel. The only precaution necessary is to regulate the strength of the current so that the copper may be precipitated as a compact and bright metallic coating, and to dry as quickly as possible. When the copper is thrown down in a spongy condition, it not only oxydizes rapidly, but it is impossible to wash out the last traces of foreign matter contained in the solution. This is well shown by number III and number VI of the second series, in both of which cases the copper was precipitated too rapidly. The solution from which the copper has been deposited contains the other elements present in the original substance. It may be easily poured off without loss, and the washings added.

It appears at least probable that nickel may be determined by electrolysis in the same manner as copper, the solution employed being the ammoniacal sulphate with excess of free ammonia. Mr. M'Candless obtained in two determinations in a commercial sample 91·26 and 91·60 per cent nickel. In both cases the nickel was thrown down completely as a bright coherent metallic coating upon the platinum.

Cambridge, Oct. 1st, 1864.

ART. IX.—*Note on the Planetary Distances;* by Prof. DANIEL KIRKWOOD, Indiana State University.

To obviate any misapprehension that may have existed in regard to the order of planetary distances proposed in this Journal for July, 1864, p. 12, it may be proper to state that by the term "radii of gyration of the primitive rings," was meant the radii of gyration of homogeneous circular rings, or, in other words, the radii of the circumferences which bisect the circular intervals between the orbits of Neptune and Uranus, Saturn and Jupiter, &c. The formula was given as empirical, in the hope that it might *suggest* a true law of nature, if not found itself in strict harmony with observation.

In the article referred to, the writer suggested the hypothesis of the contemporaneous formation of the different members of our planetary system. Some additional considerations bearing on this subject may not be destitute of interest.

The radius of gyration of the sun, regarded as a homogeneous sphere, is 0·002936; the unit being the mean distance of the sun from the earth. If we designate by K the principle radius of gyration when the rotation period was the same as Neptune's orbital revolution, we shall have

$$25^{d}\cdot325 : 60126^{d}\cdot7 :: (0\cdot002936)^{2} : K^{2}\text{; whence } K = 0\cdot143.$$

But the radius of gyration of a *homogeneous* sphere filling Neptune's orbit is 18·974. Hence we infer that before the formation of the planets the condensation of the solar spheroid had advanced much more rapidly about the center than toward the surface.[1] The consequent tendency to unequal velocities of rotation would produce a divellent force, thus separating the nucleus from the outer parts, and also breaking up these exterior portions into distinct zones or annuli. As subsequent condensation would, to some extent, increase the rotary velocity of the nucleus, the primitive circle of division between the latter and the surrounding nebulous mass must have been somewhat exterior to the present circle of equilibrium between the centripetal and centrifugal forces. The points in the revolving nebulous matter at which the planetary formations would originate, were determined by certain conditions which in the present state of our knowledge we are unable to define. It is not, however, an improbable assumption that the intervals between the orbits of such nuclei would be arranged in regular order. This, in fact, is found to be the case, at least approximately, in regard to Ju-

[1] This fact, together with its cause, as the writer has recently learned, was stated by Prof. Stephen Alexander, at the Montreal Meeting of the Am. Assoc. for the Adv. of Sci.

piter's satellites. The differences between the radii of the orbits, commencing with the greatest, are,

11·64811, 5·72677, and 3·57494.

But while the first is almost exactly double the second, the second is less than double the third. In other words, as in the case of Mercury, the theoretical distance of the innermost satellite is somewhat greater than the actual distance. Now, if the *original* intervals constituted an exact geometrical progression, can a probable reason be given for the *present* deviation? We reply (1) that in the case of the primary system considerable masses of meteoric matter are believed to be still revolving within the earth's orbit; portions of which are exterior to Mercury, and some even beyond the orbit of Venus; (2) that if the Nebular Hypothesis be true, the primitive constitution of the secondary systems was probably similar; (3) that in any system an increase of mass in the central body would diminish the orbits of the satellites; and (4) that the contraction of meteoric rings until portions originally *exterior* to a given orbit had become *interior*, would obviously have the same effect. In the manner thus indicated, the distance of the innermost satellite may have been diminished, and thus the original interval between the first and second orbits increased. The same explanation applies to Mercury.[2]

With an interval = 11·64811 between the third and fourth satellites, and a ratio = $\frac{1}{2}$, we find 3·70213 as the limit at which the central body separated from the exterior mass. This is a little beyond the present circle of equilibrium between the centrifugal and the centripetal forces, the radius of this circle being 2·299.

THE SATURNIAN SYSTEM.

In the primary system[3] and also in that of Jupiter, we have found that the original circle of separation between the nucleus and the surrounding mass was a little exterior to the present circle of equilibrium between the two central forces. This is what ought to be expected, as the contraction of the nucleus would accelerate the rotation. For Saturn, however, we found the primitive circle of equilibrium *less* than the present;[4] but in this calculation no allowance was made for the contraction of

[2] This explanation, it must be confessed, is apparently inconsistent with the conclusions of Leverrier in regard to Mercury's motion. The distinguished director of the Paris Observatory "has found that Mercury's mean motion has gone on diminishing; as if the planet were, in the progress of his revolutions, receding further from the sun. This is explained, if we suppose that there is, in the region of Mercury, a resisting medium which moves round the Sun in the same direction as the Planets move."—*Whewell's Hist. of the Ind. Sci.*, 3rd ed., vol. i, p. 560.—Perhaps no satisfactory determination of the facts can be reached without long continued observations.

[3] This Journal for July 1864, p. 13.

[4] Ib., p. 15.

the orbits of the innermost satellites. If the radius of gyration for Mimas and Enceladus was diminished in a ratio equal to the mean of those obtained for Mercury and the innermost satellite of Jupiter, it was originally 3·8720. Adopting this value and interpolating the two missing terms, we have the following elements conforming to our hypothesis:

SATELLITES OF SATURN.

Names.	Distances.	Rad. of Gyr. of Intervals.	Intervals.
I. Japetus,	64·359	54·9764	
———	43·6203		32·0070
II. Hyperion,	25·029	22·9694	
Titan,	20·706		12·4154
III. ———	11·958	10·5540	
Rhea,	8·932		4·8150
IV. Dione,	6·399	5·7390	
Tethys,	4·9926		1·8670
V. Enceladus,	4·0319	3·8720	&c.
Mimas,	3·1408	&c.	0·0000
	&c.	Limit=2·6862	

The ratio of the ascending series of intervals is 2·578+. These ratios in the systems of Jupiter and Saturn are to each other inversely as the orbital velocities of the two planets. The distance from the center of Saturn at which a satellite would complete its orbital revolution in the present period of the planet's rotation is 2·0075. The distance at which the nucleus originally separated from the surrounding mass has been found (theoretically) somewhat greater, as it ought to be, viz., 2·6862.

If, then, the arrangements of the Saturnian System should not be admitted as confirmatory of the empirical order of distances, we may at least conclude that it is not incompatible with it. The *exact* coincidences are of course produced by the interpolation of the two terms. With these, our formula gives three equations with three unknown quantities, and, consequently, whatever the distances of the known satellites, the roots of these equations, whether real or imaginary, must, in an algebraic sense, satisfy the conditions. At the same time it is easy to perceive that these algebraic results *might be* decidedly unfavorable to the proposed hypothesis.

The tendency in a rotating nebula to unequal angular velocities, resulting from the increased rapidity of condensation from the equator toward the center, may, perhaps, also account for the phenomena of spiral nebulæ. If, in a contracting mass of vapor, a free motion of the particles among themselves be established before the centrifugal force becomes equal to the centripetal, a spiral convergence, like that of No. 51 in Messier's catalogue, would naturally ensue.

Finally, if the original constitution of the Solar and Saturnian Systems was such as we have supposed, can any probable reason be given why the satellites of Jupiter should be found an exception?[b] It may be worthy of remark that if these bodies, or the rings from which they were derived, were originally double, the proximity of the members was such that they might be brought into collision by perturbation, while in the gaseous state. The ratio, moreover, of the ascending series of intervals is considerably less than in the case of Saturn, and much less than in the primary system.

ART. X.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxxv, p. 60, 1863.)

No. 281. *Carex conjuncta*, Boott, Illust. No. 282.
—— *vulpina*, Sullivant, Carey, Dewey.

Spica composita oblonga; spiculis ovatis sessilibus superné staminiferis et aggregatis, inferné laxis sæpe sub-ramosis et sub-remotis, bracteatis; fructibus *distigmaticis* ovatis acutis brevi-rostratis bidentatis stipitatis et interdum sub-cordatis margine subscabris divergentibus, squamam ovatam acutam paulo superantibus; spica plantaque pallido-viridi; culmo 1–3 pedali late-triquetri, cum foliis sub-radicalibus et longis.

This form was discovered some years since in Ohio, and named by Mr. Sullivant, after the European plant, C. vulpina of Linnæus. So it was held to be by Mr. Carey and others, and under that name described in this Journal, vol. viii, p. 348, 1849. Dr. Boott, however, thought it differed from C. vulpina, and gave it the name above. The figures in the Illustrations, No. 282, were taken from plants collected near Columbus, Ohio, and fully resemble those from Menard Co., Ill., now before me.

In the Illustrations, Dr. Boott gave the following particulars in which this plant differs from the European *C. vulpina*, viz: a very "acute-angled and flaccid culm," sheaths of the leaves transversely corrugated, spike more lax and pale, inflorescence of a pallid color, and its general appearance and habit. These or similar characters show the great difference between C. cephalophora and C. Leavenworthii, and doubtless should be considered important and distinguishing, even when, as in this case, the characters taken from the inflorescence are so nearly the same.

This species resembles C. sparganioides, but its *nerved* fruit separates it; as does the form of the fruit from C. stipata. The specimens of C. vulpina, *L.*, from England, Germany, and Swe-

[b] For some suggestions in regard to Mercury, see this Jour. for July, 1864. p. 13.

den, have an inflorescence more *compact* and *reddish brown;* and the plant generally has a *deeper green*, and is also *stouter*.

282. *C. glabra*, Boott, Illust. No. 229.

Spicis distinctis cylindraceis pedunculatis; terminali staminifera, interdum ad apicen vel basin pistillifera, vel in medio; spicis pistilliferis, 3–4, sub-laxifloris, bracteatis, infernè longo-pedunculatis et nutantibus; fructibus *tristigmaticis* ovalibus sub-inflatis et infernè teretibus, supernè conicis brevi-rostratis, bidentatis glabris nervosis, squamam oblongam sub-acutam vel lanceolatam multo superantibus; planta pallida et glabra.

Culm 1½ foot high, slender, erect, leafy toward the root; staminate spike slender, sometimes with a few fruit at the apex, in the middle or at the base; pistillate spikes 3–4, cylindric, slender, rather loose flowered, on slender peduncles with sheathing bracts which equal the culm nearly; stigmas 3; fruit oval, tapering below and conic above, smooth and glabrous, nearly twice longer than the oblong acutish scale which is white on the margin and green on the back: whole plant light green.

This plant is the well-known *glabrous* form of C. flexuosa, *Schk.*, the C. debilis, *Mx.*, and blended with it till Dr. Boott separated them in 1860. More than 40 years ago it was in our country commonly labelled from fig. 124, Schk.

283. *C. Magellanica*, Lam. 1789, and Schk., 1802. Boott, No. 199.
C. limosa, var. irrigua, Wahl. 1803; Dewey, 1826.
C. paupercula, Mx. 1803, and Torrey, 1843, and Boott.
C. irrigua, Smith, English Botany, 1845.

Spicis pedunculatis cylindraceis vel brevi-oblongis ferrugineis; terminali vulgo staminifera brevi, raro apicen pistillifera; spicis pistilliferis 1–3, subremotis recurvo-pedunculatis, bracteatis, laxifloris, interdum basin stameniferis; fructibus, *tristigmaticis*, longo-rotundis obovatis sub-triquetris vix rostratis ore integris stipitatis, squama ovato-oblonga acuta vel lanceolata brevioribus; culmis, foliis, bracteisque glaucis.

The stamens at the base of the fertile spikes have not been noticed by Torrey, Carey, and others generally who have collected the plant. They seem to me to be less common than they appeared to Dr. Boott, though I find them, and they were found by Schk. The American plant seems to be a larger and stronger plant than the European, even to the European botanists.

Though the resemblance of the figure of C. Magellanica in Schk. to C. limosa may have been noticed, it was reserved for Dr. Boott to prove, by examination of the specimens of Lamarck, their identity with common C. irrigua of Europe and North America. Lamarck's name, being the oldest, must be the accepted name of this species. It was briefly described in this

Journal, volume x, page 42, 1826, under the above name of Wahlenberg.

In cold marshes in the Northern and Western States, and Canada.

284. *C. rariflora*, Smith, Eng. Bot., 1845.
C. limosa, var. rariflora, Wahl., 1803 and Schk., 1812.

Spica terminali staminifera brevi; spicis pistilliferis 1–3, sæpe 2, linearibus brevibus laxifloris pedunculatis et nutantibus, bracteatis vix remotis; fructibus, *tristigmaticis*, ovatis oblongis vel ellipticis triquetris subcompressis obtusis apiculatis ore integris, squamam ovatam obtusam vel acutam sub-fuscam subæquantibus; planta glauca brevi et gracili.

White Mountains, N. H., on borders of Blue Pond—Dr. Barratt: northern parts of Europe and America. As a variety of C. limosa, this was very briefly described in this Journal with the preceding species.

In the *Summa* (p. 233, 1846) of the acute Fries, he has spoken of this C. rariflora, "quasi forma reducta," *as if a reduced form*, "scarcely a span high," of his preceding species, C. Stygia; but C. Stygia seems more nearly related to C. limosa, *L.*, than is C. rariflora. Hence, we find that Lang, in his *Scandinavian Carices*, admits both as proper species; and he will doubtless be approved by others.

Note. The following rare variety of C. limosa *L.*, was discovered the last summer by Rev. John A. Paine, of Utica, who has been actively and successfully engaged in looking up the *rare* as well as the common plants.

C. limosa, L., var. *Painei*, Dew.

Culm with the terminal spike staminate, and one pistillate spike of the common size and form on a slender or hair-like peduncle, from five to nine inches long, rising from near the root. In one instance there was a single pistillate spike on the culm and near the staminate spike, and the long radical pedunculate spike as before. The common form and the variety here described were found growing on different culms from the same root.

This is a very curious variety, and seems not to have been discovered before. Dr. Boott says of the lowest pistillate spike of C. Magellanica, "rarius subradicalis;" but this is on C. limosa and is truly radical, while he has figured that as subradical and short pedunculate instead of very long.

285. *C. mirata*, Dew.

Spicis 3–5, longo-cylindraceis inclusè pedunculatis longe-folioso-bracteatis; spicis staminiferis 1–3, sæpe 2, approximatis, in-

terdum ad basin vel erga apicen pauco fructiferis, cum glumis longis arctis attenuatis scabro-subulatis; spicis pistilliferis 1–2, laxifloris suberectis sub-remotis ad apicen vulgo staminiferis; fructibus parvi-ovatis longo-conicis vel lanceolatis vix inflatis nervosis vel striatis longo-stipitatis divergentibus rostratis, rostro profundi-fisso bicuspidato interdum bifurcato vel bidentato; glumis fructiferis lineari-lanceolatis scabro-subulatis, fructu superno spicæ brevioribus, fructum inferiorum æquantibus, atque fructus infimos plus duplo superantibus: culmo superne scabro, inferne obtusi triquetri et lævi; foliis bracteisque nodosis et margine scabris.

Culm 15–20 inches high, stout and stiff, triquetrous, obtuse-angled and smooth below the lowest bract, but rough above it, with long and broad leaves from the base and long leaf-like bracts, longer than the culm, and the lower the longer, both net-veined and very scabrous on the edges; staminate spikes 1–3, commonly 2, long-cylindric, near but the lower more remote, covered with long narrow bristle-form glumes attenuate and rough-subulate, with a few fruit at their base or toward the summit; pistillate spikes 1–2, often 1, long cylindric, suberect, exsert-pedunculate, at their summit staminiferous: stigmas three; fruit round and small ovate, long-conic, diverging, scarcely inflated, long-stipitate, with a beak deeply bifid, sometimes bicuspidate or bifurcate or bidentate with scales ovate lanceolate, rough-subulate, shorter than the fruit at the upper part of the spikes, equalling the fruit along the lower, and sometimes several, four to ten, of the lowest scales even twice as long as the fruit. Plant pale green.

In Greece, eleven miles west of Rochester and six south of Lake Ontario, in 1829, by Dr. S. B. Bradley. It was not named for a long time, as its anomalous characters made more specimens desirable. But no others have been discovered in the vicinity of this locality, which the clearing of the forest destroyed. In Belleville, Canada West, in 1863–4 by John Macoun, Esq. One form, sent by him, accords with the above description, especially in the varieties of the fruit, of the rostrum, and the very long scale of the fruit at the base of the lower pistillate spike.

Dr. Boott in Illust. No. 58, refers this species to C. aristata, *R. Brown*, by a mistaken[1] reference to my description of C. aris-

[1] His reference, "C. mirata, *Dewey*, Sill. Jour., xxvii, 240, v, 49, 48; Wood's Bot.," should have been *C. aristata*, Dewey, *Sill. J.*, xxviii, 240, vol. xlix, 48 p.; *C. mirata*, Dewey, *Wood's Bot.* Not only was the volume wrong, but *mirata* was unintentionally substituted for *aristata* by Dr. Boott, which changed the whole subject. Indeed the full description of C. aristata, Sill. J., vol. xxviii, 1835, referred to by Dr. Boott, was printed years before I had heard of C. mirata, proving that the description was of C. aristata evidently, and of no other, it may properly be added, as Dr. Boott implies in 1858.

If the "Note," Sill. Jour., xlix, 48, 1845, is noticed with care, it is obvious a *mistake* is made, because the last sentence *denies* what the preceding *asserts*. It will

tata, which has no connection with any other specimens, and by that self-destructive "Note," vol. xlviii, p. 49.

When No. 58 was published, Dr. Boott had not seen any specimen of C. mirata; but since that time he has seen two specimens of it, which I forwarded to him, and one of which was a present to him from Dr. Bradley. Circumstances led me to suppose he had changed his opinion in consequence; but his death, so unfortunate for this science, has prevented the publication of the 4th Part, which was to contain it and was known to be nearly ready for publication. What measures can be taken to secure its being printed and circulated?

Another form, judged by Mr. Macoun to belong to this species, has been provisionally named

C. mirata, var. minor, *Dew.*

Staminate spikes 2–3, often 3, long and short, and scales slightly larger; pistillate spikes 1–2, some shorter with stamens at the apex, or below it, or both, as in C. mirata, with fruit slightly more inflated, and its scales generally shorter than the fruit; plant older, with brown spikes and scales.

In Belleville, C. W.; Mr. Macoun.

Note. C. rostrata, *Mx.*, commonly obtained from ponds near the base of the White Mts. N. H., was found most abundant by Rev. Mr. Paine of Utica, in August last, at a pond or small lake near the well known Bald Rock or Mt. in the N. E. part of Herkimer Co., at the west foot of the Adirondack Mountains. Muhlenberg had not seen it, as he referred this name, *rostrata*, to his C. tentaculata. It was not recognized from the time of Michaux to 1840, when it was described in this Journal, vol. xxxix, p. 52.

C. lenticularis, *Mx.*, was found also by Mr. Paine along the chain of the "Eight Lakes," in Herkimer Co.

C. Houghtonii, *Torr.*, has been found, the last season, 60 miles north of Belleville, by John Macoun, Esq., an active and discriminating botanist. The same species has been found, the last two seasons, by Rev. Mr. Blake, of Gilmanton, N. H., and in Milford, north of Bangor, Me. With great pertinence was it named by Dr. Torrey.

be rectified by the insertion of C. aristata, so as to read, "For description and figure of C. aristata, *R. Br.*, refer to vol. xxviii, p. 240, of this Journal; tab. v, fig. 67. This species the fig. 57 finely represents, while it is very unlike C. mirata, as is that description in vol. xxviii. So certain is it, that the "Note" assumes the great difference between C. aristata and C. mirata, instead of uniting them.

ART. XI.—*On the Action of Ozone upon Insensitive Iodid and Bromid of Silver;* by M. CAREY LEA, Philadelphia.

A STATEMENT has recently appeared in the French scientific Journal *Les Mondes*, from which it has been copied,[1] to the effect that ozone is capable of giving sensitiveness to insensitive iodid of silver. Mr. J. P. Kaiser states that he has found that certain vapors, such as the vapor of benzine, exercise a powerful effect of this kind, converting one insensitive variety into one that was highly sensitive: that he attributed this effect to the ozone produced by the contact of this vapor with atmospheric air:—that he therefore experimented with air ozonized by a galvanic induction apparatus, and found the same results produced, but in a much more marked degree.

This statement was not altogether the first suggestion of the matter which I had seen. A French Photographic Journal narrated, a few months back, a case in which a photographer, whose plates had refused to afford any image at all, restored their sensitiveness and got them into satisfactory working order by simply generating ozone in the room. To this latter statement, I was disposed to attach little importance. But the explicit and circumstantial one just above referred to, was a different matter.

Those who put forth these statements do not seem to have been aware of their true importance. Not only would such a fact be of great theoretical interest, but its practical applications would be immense, especially in reference to out-door photography, the easy execution of which on wet collodion would at once have become practical. For plates could be coated with insensitive iodid of silver in ordinary light, (say, for example, after Mr. Sayce's formula). This plate could be placed in an ordinary dark slide; and a vessel containing the means of generating ozone could be placed in the camera. On withdrawing the dark shutter from the slide, the plate would be uncovered, and exposed to the ozonized air of the camera. In due time the lens would be uncovered, and the image allowed to fall on the now sensitive surface. The development would then be effected in one of Weiske's developing cases, and thus the whole process would be reduced to the utmost simplicity.

In view, therefore, of the interest which attaches to this question, I was led to make the following trials. In place of using ozone generated by electricity, as done by M. Kaiser, I preferred to generate it by chemical means, viz: by phosphorus, and also by the action of sulphuric acid on chameleon mineral. As the

[1] See British Journal of Photography, 1864, p. 392. Also Phot. Mittheilungen, Nov. 1864, in which it is suggested that ozone acts by destroying a trace of alkaline iodid present.

source of the ozone was not the same as that used by M. Kaiser, my results cannot be considered as strictly controlling his, but the action of ozone from the two sources is so similar that we should naturally expect similar results in the two cases.

A. Ozone by phosphorus.

The ozone was generated in a large bell glass, and the experiments were not commenced till paper impregnated with starch and alkaline iodid exhibited an immediate and strong ozone reaction.

I. *Action on Iodid of Silver.*

Paper was plunged into an ordinary negative bath, and dried. Strips were immersed in solution of iodid of potassium, and without leaving them in too long, were next thrown into clean water and washed.

(1.) A piece of this paper, thus imbued with washed insensitive iodid of silver, was placed in the ozone apparatus for two minutes. It was then exposed to diffuse daylight for six seconds. The application of an iron developer produced no darkening whatever. A longer exposure to light was also without effect. The paper was just as insensitive as before being exposed to the ozone.

(2.) The effects of a longer exposure to the ozone were next tried. The paper, prepared as before, was exposed for half an hour to the action of the ozone, and exposed to a moderate diffuse daylight for twenty seconds. The iron developer produced no effect whatever.

(3.) Same as (2), but exposed to light for thirty seconds. Result as before.

(4.) The paper was prepared as before, but, after immersion in the solution of iodid of potassium, the washing in water was omitted, and the strip was placed in the ozone apparatus just as it left the solution of iodid. The paper immediately changed to a deep chocolate brown, while still in the ozone apparatus. This effect was at once attributed to the action of the ozone on the free alkaline iodid, but to place the matter beyond doubt, the paper was thrown into a solution of hyposulphite of soda, which instantly bleached it.

It seems hardly likely that this reaction, so well known, could have been mistaken for an indication of sensitiveness to light. Perhaps, if the exposure to ozone had been conducted in diffuse daylight instead of in a dark room, a careless experimenter might have been misled, by the similarity of the chocolate-brown color produced to that so often occurring in photography, into a misapprehension of the agency at work, and might have supposed that the ozone had rendered the insensitive iodid of silver sensi-

tive to the diffuse light of the room. Such an error seems, however, unlikely.

(5.) Paper was prepared as before, was exposed fifteen minutes to the action of ozone, then one minute to the light of a strong argand gas burner, one half being protected from the light. An iron developer was then applied. No effect was produced, nor could the side exposed to the light be distinguished in any way, after the application of the developer, from that which had not been exposed.

[It was previously ascertained that a sensitized collodion plate placed in the same position as respects the same light, was powerfully impressed in 20 seconds, or one-third the time just mentioned.]

(6.) Paper was prepared in the same manner, but after placing in the solution of iodid, and washing, it was dried, and in this condition exposed to the ozone for fifteen minutes. Then to the same gas light for one minute, partly covered. Result same as number 5.

(7.) After treatment with silver and iodid, and washing, the paper was exposed to ozone for fifteen minutes, then to the same gas light as in (5) and (6) for one minute, partly covered. It was then developed with pyrogallic acid to which nitrate of silver and citric acid had been added, and this was kept on till the paper was blackened by the deposition of silver. Not the slightest distinction could be traced between the parts that had been exposed to light, and those that had been produced by thick yellow paper.

II.—*Action on Bromid of Silver.*

(8.) In order to give a greater variety to the experiments, the bromid of silver submitted to trial was not disseminated through paper as was done with the iodid in the foregoing trials, but was used in a collodion film. For this purpose a collodion was made somewhat similar in character to that proposed by Mr. Sayce, in which the bromid of silver is suspended in the collodion with its sensibility destroyed by the presence of a small excess of alkaline bromid. The proportions used were as follows:

Ether,	½ ounce.
Alcohol,	1 "
Pyroxyline,	10 grs.
Bromid of ammonium,	9 "
Nitrate of silver,	13½ "

These proportions leave a slight excess of bromid of ammonium present, which ensures that the bromid of silver is in the insensitive form.

(9.) A portion of this collodion was poured on glass and ex-

posed to a highly ozonized atmosphere for three minutes. It was then exposed to a large gas light for fifty seconds. On the application of an iron developer no darkening was observable.

(10.) In order to afford a term of comparison, a film of ordinary collodion was sensitized in the negative bath in the usual manner, and was exposed to the same light for twenty seconds. On the application of the same developer it was strongly darkened.

(11.) The same collodion as in (9) was exposed to the same ozonized atmosphere for 45 minutes. It was next exposed to the same gas light as (8) and (9) for thirty seconds. The application of the developer produced no darkening.

(12.) Same as (10), but exposed to light for one and a half minutes. Result as before.

(13.) Same, but exposed to light for three minutes. Result as before. In no case was the slightest reducing effect produced.

B. Ozone by chameleon mineral.

Action on Bromid of Silver.

The bromid of silver was used in the same form as before, viz: in collodion containing a small excess of bromid of ammonium. I should have mentioned before that the insensibility to light of this argento-ozonized collodion was first carefully tested and proved. Ozone was generated in a closed box by the action of undiluted sulphuric acid on chameleon mineral; the vessel containing which was set aside for a short time to let the purple vapors pass off. It was then set in the box, and the condition of the atmosphere was examined from time to time by appropriate test paper.

(14.) Before commencing with the ozone trials, a piece of glass was collodionized with ordinary collodion and sensitized in the negative bath. It was then exposed to a weak diffuse daylight for thirty seconds, after which it immediately darkened under the developer. This was to determine the action and power of the light in question.

(15.) The collodion containing insensitive bromid was exposed to a weakly ozonized atmosphere for ten minutes. It was then exposed to the same daylight as the ordinary sensitive plate mentioned in (14), and for the same time. An iron developer was then applied, but not the slightest indication of sensitiveness was observable.

(16.) The last experiment was repeated under the same conditions, but with a treble exposure to the action of the same ozonized atmosphere, viz: thirty minutes instead of ten. The exposure to light was the same, and the insensitiveness the same.

(17.) Instead of a weakly ozonized atmosphere, as in the last

two cases, a strong one was prepared, and a plate similarly prepared with insensitive argento-bromid collodion was placed in it ten minutes. It was then exposed to the same light and treated in the same manner as in the experiment immediately preceding, and with the same results:—not the slightest indication of sensibility.

In this last experiment, the atmosphere was ozonized sufficiently to render iodid of potassium and starch paper instantly blue. In the two previous, this reaction showed itself more gradually.

In remarking upon the experiments just detailed, I may observe in the first place that the ozone was unquestionably always present in such strength as to bring out its marked chemical effects. This was demonstrated not only by its action on the characteristic test paper, but also by its chemical action exhibited in experiment (4).

Again, the experiments were tried in the most varied ways. The ozone was produced in two different manners, and of varied strength. The surfaces to be tested were also exposed for widely varied times, from two minutes to forty-five.

The nature of the light was also varied, experiment having been made both with daylight and artificial light. The time of exposure to light was very various, and in several cases, after the first results had been noted, the paper or film still wet with developer, was carried into the light and exposed for some time, to see if any faint sensibility existed and would manifest itself by the prolonged action of light. No such result appeared. The experiments included both iodid and bromid of silver, and both paper (of which two different sorts were intentionally used) and collodion were used as the vehicles for the silver compound.

The result appears to show pretty clearly that ozone has no power of giving sensibility to insensitive iodid or bromid of silver formed in the presence of excess of alkaline iodid, whether the excess be left present, as in the bromid experiments, or be removed, as in those made with iodid. Or at least that this is true in respect to ozone produced in the two manners which I have described. If Mr. Kaiser has really succeeded in sensitizing plates in the manner described by him, I think that result might possibly be ascribable to some direct effect of the induced electricity which he employs, and not to the ozone produced. But the description of his experiments which has reached me in the *British Journal* is so brief (no details whatever are given) that the above can only be considered as a mere suggestion of an explanation of the opposite results obtained by him. The whole subject is an interesting one, and the effect of all the forms of physical force, especially electricity and heat, on the silver haloids might no doubt be advantageously studied.

ART. XII.—*Prize for applications of the Electric Pile.—Heterogeny.—Influence of Light on Proto-organisms.—Association for the Advancement of Meteorology.—On the intensity of action of the Solar disk.—Acclimation of Salmon in Australia.—Production of the Sexes.—Cutting of the Isthmus of Suez.—First idea of Electric Telegraphy.* Correspondence of Prof. J. NICKLÈS, dated Nancy, France, Oct. 20, 1864.

Prize for applications of the electric pile.—The prize of 50,000 francs, which was founded in 1852 by the Emperor Napoleon, has just been decreed to Ruhmkorff, for the induction apparatus known as "Ruhmkorff's coil," the mechanism of which we were among the first to make known.[1] The committee, consisting of Messrs. Pelouze, Rayer, Serres, Becquerel, H. St. Claire Deville, and others, was presided over by Dumas. We make the following extracts from his report:

"Mr. Ruhmkorff was at first a workman for some of our best constructors of physical apparatus, afterward had his own workshop, and finally became head of a house of constantly increasing celebrity.

"His education was gained, little by little, through reflection, study, and the lectures of certain professors heard as it were by stealth in his occasional hours of leisure. Modest, of unyielding perseverance, and of a self-devotion which has earned for him the highest encomiums, Mr. Ruhmkorff will ever remain the type of his class—a model for the numerous intelligent workmen who fill the higher order of workshops (ateliers de precision) of Paris. To those, who, like him, know how to control their desires, who faithfully strive for perfection in work and clearness in conceptions, who bend their attention to one object, and labor untiringly until a high superiority is gained and also for themselves the satisfactions of a ripe age, the compensation for the sacrifices and privations of youth will not be lacking in a country where, more than ever, merit finds recompense.

"Since 1851, Mr. Ruhmkorff has devoted himself to the construction and perfection of his apparatus, and he has ended by securing for it his own name, by giving it a scientific value which no one contests, and by rendering it of so great power as to become a means of numerous practical applications.

"The Ruhmkorff apparatus unites the two forms of electricity, which had been separated as by an abyss—the old machine electricity, characterized by a capability of producing sparks and by strong tension, and the electricity of the pile, characterized by very feeble tension and by its inability to produce true sparks.
* * * *

[1] This Journal, 1853, xv, 114; 1862, vol. xxxiii.

"If Franklin's discoveries placed beyond doubt the identity of mechanical electricity and lightning, there remained, nevertheless, among the phenomena which accompany storms, many circumstances the explanation of which was yet inaccessible to science. We must therefore regard as a valuble acquisition to meteorology the observation that the spark of the Ruhmkorff apparatus consists of two parts—an instantaneous line of light and an areola of measurable duration. The magnet divides the latter; a breath, or any body in motion, draws it out, and the electric spark thus divided continues its route in these two directions at once, as long as the passage of the current continues uninterrupted.

"In a vacuum, the electric spark develops light, and that developed by the Ruhmkorff spark takes different colors in different gases; it brilliantly illuminates fluorescent bodies, and divides itself into parallel bands separated by dark spaces perpendicular to the axis of the recipients. These colored luminous bands obey the action of the magnet which attracts or repels them, and which impresses upon them, at the pleasure of the operator, those movements of translation or of rotation by means of which De la Rive[1] has reproduced the appearances observed in the Aurora Borealis, justifying, thus, the analogy recognized between the electric light produced in a vacuum and that of the polar auroras.

"Glass tubes illuminated by the same means give out a light sufficiently bright to be used in mines that are liable to explosion: they also serve under water as a lamp for divers—and in surgery, to carry into the back part of the mouth, and other cavities of the body, a light which gives no sensation of heat.

"The spark of the Ruhmkorff apparatus inflames combustibles, and explodes gaseous mixtures; and it has thus supplied to Lenoir's gas-engine the means necessary for producing the successive inflammations to which it owes its mechanical power.

"The working of quarries, the boring of tunnels, the explosion of heavy charges in mines, give to-day regular employment to the Ruhmkorff apparatus.[2] Mines had been exploded previously by the aid of the pile; but the Ruhmkorff apparatus has left all other modifications far behind, by reason of the small number of elements which it requires—three instead of one hundred—the power of its spark, and finally the possibility of exploding eight or ten mines at once. In the expedition to China, in 1860, the Ruhmkorff apparatus was used to blow up, by means of eight mines simultaneously exploded, the principal fort of the Peiho, as well as the iron stockade at the bottom of the river."

Dumas then reviews the principal applications of electricity to Mechanics, such as the *automatic break* of Achard, the weaving-

[1] This Journal, 1853, vol. xvi. [2] This Journal, 1854, vol. xviii.

looms, the pentagraph of Caselli, the writing telegraph of Prof. Hughes, etc. He then spoke of illumination by electricity, and mentioned the electric regulators of Staite, Serrin, and Dubosc, the illumination of light-houses by induction currents, observing that successful experiments have been made with it by the Light-house Board: at Havre, upon Cape de la Hève it erected, several years ago, an electric lighthouse equivalent to 3000 Carcel burners, by the side of one lighted with oil; the electric light was readily distinguished, by its brilliancy and intensity, from its neighbor which appeared red.

Under *Medical electricity* there was a reference to the observations of Dr. Duchenne of Boulogne and those of Middledorf, of which we have given an account when treating of Galvanocaustic, and according to which, by means of platinum wires brought to incandescence, the tissues may be divided, and the removal effected of polypi and tumors from organs that are deeply seated or otherwise difficultly accessible.

A new competition will take place, five years hence, to which all applications of electricity will be admitted, whether to medicine, the mechanic arts, or industry, without distinction of origin or of nationality.

Heterogeny.—The discussion which has existed for several years between the partisans of spontaneous generation and their opponents, is entering upon a new phase. The two parties desire to unite and experiment together, for it is in that direction that a beginning must be made. Thus it has happened, as in politics, at first fighting, then, conferences or conventions.

Whatever be the opinion on this great subject of generation, it is impossible that new and instructive facts should not be brought out by this convention, in which chemists of known skill are assisted by such physiologists as Pouchet and Joley. There is every reason to believe that each party holds some truth, and that their differences have arisen from their different methods of experimenting. At any rate, there is no opportunity for temper in the debate.[4]

Influence of light upon the production of proto-organisms.—One element of the solution of this question of heterogeny resides in the sun's light, which produces some effects that are not known to result from artificial light or heat, as has been already made known by Messrs. Pouchet and Ch. Morren. Some new facts in

[4] Since the above was written, several meetings have been held at the Garden of Plants in Paris. Nothing has resulted from this convention except the very sad fact that theological considerations seem to have got the better of the attentive observation of facts, just as in the time of Galileo. There is, then, nothing to do but to observe that the savants have all agreed that reason, which doubts, observes and studies, has nothing in common with that kind of faith which believes without examination and without proofs.

their support have been observed by Professor Montegazza at the University of Pavia.

Two female frogs were quickly killed, by the destruction of the spinal marrow, and placed in two glass vases each of which contained 115 cubic centigrams of well-water perfectly transparent and free from all foreign bodies. One of these vases was left in diffused light, the other was placed in a box which did not permit a single ray of light to reach it.

The experiment was continued seventeen months. It is necessary, before giving the results of this long trial, to state that every time that the two vases were compared, they remained exposed to the air for an hour or more, and that the air was renewed at each observation. Germs could, then, easily fall in; and yet the results were very different in the two cases.

The following are the observed results:—

1. Two identical bodies exposed to free air may present very different phenomena of putrefaction, according as they are exposed to the influence of the light, or shut off from it.

2. The chemical and biological phenomena of the two forms of putrefaction are very different, that is, we have in each case some special chemical products, and some peculiar animal and vegetable productions.

3. In darkness, there was a marked tendency to the production of vegetable organisms and very simple infusoria; the frog that underwent putrefaction while shut off from the light produced only some Mucedines, Monads and Vibrios; while the other afforded a very complicated fauna—Bacteriums, Vibrios, Spirellas, Monads of different species, Amœbas, Kerones, Alysum, Enchelides, Trachelius,—and finally Infusorians still undescribed, much resembling the Zoosperms of the Tritons.

4. The abundance and superior organization of the Infusoria depend much more upon the progress of the putrefaction than upon the amount of putrescible matter. The more simple species always appear first.

5. The production of species of Bacterium takes place many times during the course of a long putrefaction.

6. When the liquid presents a new fauna, the new species are from the outset represented by a number of individuals at once; from one day to the next, they are simultaneously produced.

7. In the course of a long putrefaction, there are some generations which endure for some days; others exist for a much longer time.

8. Rapid changes in the chemical composition of a putrescible liquid are always, or nearly always, followed by new sets of animal and vegetable microscopic life.

9. When circumstances are little favorable to heterogeny in a very long putrefaction, there may be intervals of time of greater or less length, in which the liquid presents no organism. Who-

ever should content himself with observing at such a moment, might say that there had been no generation; while some days before, or some days after, there had been, or there would be, a very abundant production of vegetables, or of animals, or of both at once.

From all the comparative tables, I select an observation made on November 20th, after more than seven months of putrefaction. Temperature at the time 10° C.

In the light.—The liquid had a slight odor of boiled meat.—On the surface many very lively Kolpodes, a great quantity of Vibrios, some Alysea.—Some Infusoria which strikingly resemble the Zoösperms of Tritons.—At the bottom, some dead Kolpodes, and some in the act of multiplying by division into two or four individuals. Many Vibrios; some Monads.

In the dark.—The liquid had a very strong odor of mushrooms.—No organisms; the whole mass liquid.

Association for the advancement of Meteorology.—This association, founded at the commencement of the year 1864, under the presidency of Le Verrier, is a great success. It meets periodically at the Observatory of Paris. It has just founded prizes for the encouragement of meteorological studies, and especially the study of the general movements of the atmosphere. All meteorologists are admitted to competition, without distinction of nationality.

The following are some of the details of the programme:

"According to the generally received opinion, the storms of the coasts of France come, already formed, from the Atlantic. An extended series of meteorological observations made over this vast sea, is, then, the necessary basis of the work proposed for the principal prize. This prize will be of 4000 francs. The memoirs must be delivered to the Secretary of the Society before December 31st, 1865.

"A sum of 3000 francs will be divided between the authors of the best observations made at sea, or in places little known in a meteorological point of view.

"Finally, two prizes, of 500 francs each, are offered for the best memoirs upon the application of Meteorology to agricultural questions. The prizes of 300 and 500 francs may consist of instruments for observation."

Upon the intensity of action of different parts of the solar disk.—With regard to the late researches by Secchi—according to which the calorific radiation of the center of the solar disk is greater than that of the borders, nearly in the ratio of 2 : 1, Mr. Volpicelli writes that the fact was very exactly observed in 1614 by Luc Valerio, a mathematician of Naples, author of a work, *De centro gravitatis solidorum*, and of another *De quadratura parabolæ per simplex falsum*. He was a professor in the Roman University, and has been called the Archimedes of his age.

In one of his letters to Galileo, Luc Valerio considers the rays proceding from the central part of the solar disk as the more active.

Analogous facts have been observed by Mr. Roscoe, according to whom the center of the disk exerts a more intense chemical action than the borders. He has also observed that the south polar zone is more active than the north.

Acclimation of Salmon in Australia.—Recent experiments carried forward by the Acclimation Society have shown that it is possible to transport to distant countries the eggs of fertile fishes. One of its members, Mr. Millet, having observed that melting ice diminished the pulsations of the young fish of the Salmonidæ and delayed the hatching of the eggs, took the idea that this method would serve for the transportation of eggs of the Salmonidæ to Australia and Tasmania. The plan has succeeded, in spite of damaged eggs; a very large number have arrived there in a healthy state, and have been deposited in the rivers. Starting from London on January 15th, 1864, the eggs arrived at Melbourne on April 5th, and at Hobart Town, (Tasmania,) on the 23d. Everything indicates success.

Similar attempts have been made in the French possessions in Algeria, the rivers of which are very barren of fish. Eggs of salmon and trout have been carried there from Huningue (Haut-Rhin), where, as we have previously seen, are found the principal basins for pisciculture. In spite of the differences of climate, these eggs have arrived safely, and have hatched in the basins prepared for them; they already begin to people the rivers. It is a result of no moderate interest to see the salmon of the Rhine and the trout of the Vosges transported to Africa, Australia, and Tasmania, and living and becoming acclimated there.

Production of the sexes.—What are the causes of the production of the sexes? This question, which has occupied the earnest attention of physiologists, has been thoroughly studied by Mr. Thury, according to whom the product is always of the male sex when the fertilization of the ova occurs at complete maturity, and is always female when it takes place at a less advanced period.

There is a very simple way of solving this problem. It is to select for experiment species that come to maturity in succession, and, that during a single impregnation, fertilize the whole series of ova which detach themselves from the ovary during a period of eight, ten, twelve, fifteen, or even eighteen days. We know, indeed, that, in the case of the hen, a single coupling suffices for the fertilization of five, six, or seven eggs which she is about to lay and which are arranged in her ovary in the order of their maturation. Now, in such a case, if the theory is exact, the first egg laid ought always to produce males and the others females without any possibility of the inversion of this order.

This is very near what has been observed by Messrs. Coste and Gerbe:

A hen, separated from the cock at the time of her first laying this year, gave five fertile eggs in the space of eight days.

The egg laid on March 15th produced a male; that on March 17th a male; that on the 18th a female; that on the 20th a male; that on the 22d a female.

A characteristic fact in this experiment is the production of a male after a female, which ought not to have taken place according to the theory. But is it only a simple exception? Or is it necessary to consider the fact a radical objection? We may learn by and by on this point, from the researches in which Mr. Gerbe is now engaged.

On the occasion of the preceding note, Flourens recalled an experiment which he made, thirty years ago.

"Aristotle had observed that the pigeon ordinarily lays two eggs, and that of these two eggs one commonly produces a male and the other a female. He wished to know which was the egg that gave the male, and which the one that produced the female. He found that the first egg always gave the male, and the second the female. I have repeated this experiment as many as eleven times in succession, and eleven times in succession the first egg gave the male and the second egg the female. I have seen again that which Aristotle saw."

Cutting of the Isthmus of Suez.—The almost certain success of the canal across the Isthmus of Suez fixes attention, more than ever, upon other projects of the kind. The cutting of the isthmus of Malacca and of that of Darien await only the completion of the Suez ship canal. In France, they are talking of uniting the Atlantic Ocean with the Mediterranean by a ship canal which would borrow part of its route from the old Southern Canal. In Holland, a society is incorporated, under the title of a "Company for the cutting of the isthmus of Holland," to establish a canal joining Amsterdam and the North Sea by a direct route, while, at the same time, the Dutch government has undertaken to establish another navigable route from Rotterdam to the sea.

A new project is presented, which, if realized, will complete the series of maritime communications in the north of Europe. It is proposed to establish a canal, navigable for ships of war and commerce, between the North Sea and the Baltic, to avoid making the passage of the Danish islands. This project, often thought of, is now very seriously considered.

Finally, they are speaking of cutting one other isthmus, and this time it is Spain which has the honor. It is proposed to pierce the Spanish isthmus in such a way that Gibraltar will be an island. The canal is to start from Trafalgar and end at Andalusia. This canal, which would cost hardly a hundred millions of francs, has for its object to prevent more than 4000 vessels

every year from lying to before the strait of Gibraltar without power to get out.

This project is just now submitted to the examination of the Spanish government, as well as to some others, such as the English and French governments.

First idea of an electric telegraph.—This first idea was brought forward in the 16th century in an anonymous work published at Rouen at that time. The question has already been discussed here. A discovery has just been made of a letter of a clerk, the abbè of Barthélemy, well known as a literary man. He writes to Madame du Dessaud, April 8th, 1772:—

"I think often of an experiment which would give us happiness. They say that with two clocks whose hands are magnetized it is sufficient to move the hands of one for those of the other to take the same direction, so that causing one to strike twelve the other will strike the same hour. Suppose that we could perfect artificial magnets so that their power would communicate itself from here to Paris: you would have one of these clocks, we would have another; upon the dial we should have the letters of the alphabet. . . . You perceive that we could thus facilitate the operation; the first movement of the hand might sound a bell which would announce that the oracle was about to speak."

Bibliography.—Among recent publications by Hachette, at Paris, are the following:

Les Assolements et les Systèmes de Culture, par M. Heuzey; 1 vol., 8vo, 540 pp.—This work contains the lectures delivered by Mr. Heuzey at the Imperial School of Agriculture at Grignon, of which he is one of the most distinguished professors. The principles which are here set forth are applicable to all countries which are to be put under cultivation.

Le Médecin des Salles d'Asile, par Dr. Cerise; 2d ed., 8vo, 200 pp.—The "Salles d'asile" of France are charitable institutions for the education of poor children. After a short historical account of the institution itself, the author proceeds to set forth the principles which should govern physicians in their supervision of these infant schools and all which relates to the care to be taken of a miscellaneous collection of children.

Abrégé de Géographie commerciale et industrielle, avec un tableau des monnaies, poids et mesures de tous les pays, par M. Gardon; 4th ed., 8vo, 400 pp.—While referring to all countries, the author deals especially with France, each department of which he describes fully as regards statistics, agricultural, mineralogical, metallurgical, manufacturing and commercial industry, importation, exportation, navigation, etc.

La terre avant le Déluge, par Louis Figuier; 4th ed., 8vo.—This is a popular work designed to initiate the people of the world in the grand phenomena of geology. It is illustrated with many colored geological maps, and more than 300 figures and ideal views of landscapes of the ancient world.

De l'Electricité considérée comme cáuse principale d'l action des eaux minérales sur l'organisme, par Dr. Scoutettin; 8vo, 420 pp. The material which mineral waters hold in solution being often in insufficient pro-

portions to account for their health-giving effects, the author has recourse to another explanation, viz: the electrical action which these waters may exert upon the human organization. We find in his work not merely a simple ideal explanation, but, on the contrary, the recital of numerous experiments made by the author upon the principal mineral waters of Europe.

L'Astronomie au XIXe siècle, par A. Boillot, 12mo, 340 pp. This work, written for universal use, gives a very faithful picture of the progress which astronomy has made from ancient times up to our day. Mr. Boillot is a very distinguished mathematician and makes astronomy his favorite study. His work has been received in France with much favor.[5]

Nancy, Oct. 20th, 1864.

ART. XIII.—*Discovery of Emery in Chester, Massachusetts;* by CHARLES T. JACKSON, M.D., Geologist and State Assayer.

IT has been said, in England, that "a good mine of emery is worth more to a manufacturing people than many mines of gold." Such being the case, it affords me great pleasure to be able to announce the discovery of an inexhaustible bed of the best emery in the world in the middle of the State of Massachusetts, in Chester, Hampden county, quite near to the Western Railroad, which with its ramifications leads to the largest armories and manufactories of metallic articles in this and the adjacent States.

For more than two years, the existence of important beds of magnetic iron ore, originally discovered by Dr. H. S. Lucas, has been known, and endeavors were made by that gentleman to organize a company for the purpose of smelting these ores. In consequence of this agitation, I was employed by John B. Taft, Esq., on the 19th of August, 1863, to examine the locality and to make report of my results to him.

On examination of my specimens of minerals, after returning to Boston, and my notes for sectional profiles of the rocky strata containing the iron ore, I found that the minerals, margarite and chloritoid, in talcose, hornblende, and mica slate rocks, indicated the occurrence of emery, the association of the rocks and minerals being identical with conditions known to exist in the localities of emery in Asia Minor.

[5] The following titles of literary works are also contained in the letter of Mr. Nicklès:

Histoire de la Littérature française, par M. Demogeot; 5th ed., 12mo, 680 pp.

Eléments de Rhetorique, par M. Filon; 12mo, 300 pp.

Les grandes scènes de la nature, par M. de Lanoye; 12mo.

Les Champs d'or de Bendigo (Australie), par M. Perron d'Arc; 12mo, 200 pp. A description of the Australian placers.

Souvenirs d'un Sibérien, par Rufin Piotrowsky; 12mo, 200 pp. This is a very touching recital of the misfortunes which afflict the unhappy Poles, for whom Europe has only barren sympathy.

I therefore called the attention of the owners of the property to these facts, and directed search to be made for emery, and that every mineral resembling it should be sent to me for examination. Little attention was paid to this prediction at the time, and not until I had invited Dr. Lucas, who resides in Chester, by personal representations and solicitations, to make the required search, the characters of emery being fully described to him.

On his return to Chester he soon learned that the miners were complaining of the great hardness of the supposed iron ore, and that no less than forty drills were dulled in boring a single hole for blasting. He then sent me pieces of this hard rock, in the belief that it was the emery I had predicted. On examination it was found to scratch quartz and topaz readily, and to have all the properties of emery; a chemical analysis proved it to be identical with the emery of Naxos.

The owners, resident in Boston, being notified of this discovery, went with me to the locality on the 11th of October last, when a full exploration of the premises was made. There are several large beds of rich magnetic iron ore at this locality, and the emery being magnetic (as it always is) had caused it to be mistaken for magnetic iron ore, and many tons of it had been smelted with the carbonate of iron and hematite in the Berkshire county iron furnaces, without a suspicion, notwithstanding its refractory nature, that the ore was emery, with only a small admixture of iron ore.

The principal bed of emery is seen at the immediate base of the South Mountain, where it is four feet wide, and cuts through the mountain near its summit, at an angle of 70° inclination or dip to the eastward. Its course is N. 20° E., S. 20° W., and its known extent four miles. Near the summit of the mountain the bed expands to more than 10 feet in width, and in some places is even 17 feet wide.

The alternations of rock in two sections are as follows, beginning to the eastward:

1. *a*, Mica slate; *b*, 15 ft. soapstone or talcose rock; *c*, 2 feet crystallized talc; *d*, talcose slate; *e*, 1 ft. granular quartz; *f*, chlorite slate; *g*, 4 ft. *Emery;* *h*, chloritoid and margarite; *i*, magnetic iron ore; *j*, hornblende rock highly crystalline.

2. *a*, Mica slate; *b*, 6 ft. magnetic iron ore; *c*, talcose slate; *d*, 6½ ft. magnetic iron ore; *e*, chlorite slate; *f*, hornblende rock, crystallized; *g*, 7 ft. *Emery*, chloritoid and margarite; *h*, magnetic iron ore; *i*, hornblende rock.

The elevation of the upper outcrop of this bed above the immediate base of the mountain is 750 feet. There are curious rounded masses of remarkably pure emery three feet in diameter in this bed entirely invested with a coat of delicate rose-colored margarite, and a thick layer of bright green chloritoid,

the investing coat being from half an inch to two inches in thickness. It is found extremely difficult to break up these masses of solid emery, drilling holes in them for blasting being very slow and laborious, and no grip can be had on their rounded sides by the sledge. A heavy drop hammer will be required to break them to pieces—or they may be cracked by fire if heat does not injure the emery.

A branch of Westfield river separates the South from the North mountain, a hill nearly 750 feet high, through the summit of which the great emery bed also cuts. On this hill the emery is more largely crystalline and less mixed with magnetic iron ore. It is more like corundum, but still contains the combined protoxyd of iron, characteristic of true emery. Its specific gravity is from 3·75 to 3·80, while that from the South mountain is from 4·02 to 4·37; Naxos emery being from 3·71 to 3·72, according to my trials of it, in comparison.

On the North mountain, three large beds of rich magnetic iron ore, distinct from the ore accompanying the emery, occur, the ore yielding 54¼ per cent of metallic iron. This ore is mined, and is smelted into bar iron by forge fires, and is also sold to mix with the hematites and carbonates of iron worked at the Lenox and Stockbridge furnaces.

Digesting the South mountain emery in fine powder with nitro-muriatic acid and sulphuric acid for a long time, it was found that 73 per cent of it was wholly insoluble in acids; and on microscopic examination the grains were seen to be translucent, and exactly like the Naxos emery prepared in the same way; but these translucent grains are readily taken up by the magnet. I therefore infer that protoxyd of iron is a chemical constituent of true emery.

Chemical analysis of the coarsely crystalline emery of the North Mountain, Chester. Sp. gr. 3·75. H.=9.

Alumina,	46·50
Protoxyd of iron,	44·00
Titanic acid,	5·00
Silica & loss,	4·50
	100·00

Emery of the South hill.[*] Sp. gr. 4·02. H.=9.

Alumina,	45·50
Protoxyd of iron,	43·00
Silica & titanic acid,	11·50
	100·00

Regarding the oxyd of iron which can be dissolved out from emery by acids as accidental, and that which cannot be so removed as an essential constituent, we shall have for the compo-

[*] The highest specific gravity of any sample from the South mountain was 4·3734.

sition of three samples of emery analysed, after digestion in acids,

	Chester, 1.	Chester, 2.	Naxos best selected emery.
Alumina,	60·4	59·05	62·3
Protoxyd iron,	39·6	40·95	37·7
	100·0	100·00	100·0

If this view is adopted, emery must be ranked as a distinct species, and not as a mere granular form of corundum or sapphire.

In conclusion, I would state that practical trials of the Chester emery, in several of the large armories and machine shops of this and the adjoining States, have proved it to be fully equal in value to the well known emery of Naxos, which I have no doubt it will wholly supplant in this country, and that it will ere long become an article of export to Europe, either in its native form, or in a manufactured state.

It may be proper to add, that John B. Taft, Esq., of Boston, in behalf of his associates, owners of the emery mine, has the sole management of the business connected with the mine.

I would express my obligations to Mr. J. L. Smith for the valuable information contained in his articles on the emery of Asia Minor and on the associated minerals of the emery localities, published in vols. x. and xi. of this Journal. Also to Dr. H. S. Lucas, of Chester, for kind assistance in the field.'

32 Somerset St., Boston, Dec. 12th, 1864.

Art. XIV.—*On the Solubility of the Sulphate of Baryta in Sulphuric Acid;* by Prof. J. Nicklès.

The sulphates of baryta, strontia, and lime, are slightly soluble, as is known, in boiling sulphuric acid. I have found that they are soluble in *cold* acid when in a nascent state.

To obtain this result, it is only necessary to throw a little chlorid of barium, or of strontium, into a sufficient quantity of monohydrated acid: the chlorid is by degrees decomposed, chlorhydric acid escapes, and the sulphate produced dissolves in the surrounding acid.

This is especially true for the chlorid of barium; but to succeed, the acid should be concentrated, and it is best to have the chlorid well dried and in powder. On adding water to the solution thus obtained, the sulphate of baryta falls as a white precipitate.

The chlorid of strontium gives the same results, and the acid solution affords a similar precipitate with water, though less in amount, since less is dissolved.

[1] On page 87, the date 19th of August should be 19th of October.

The sulphate of lime is still less soluble in sulphuric acid, and the solution takes several days to become limpid. Moreover, water does not then becloud it; a light precipitate is obtained with alcohol.

The solubility of these sulphates in sulphuric acid, is then the reverse of their solubility in water, except for the sulphate of strontium, which, in both cases, is a mean between those of the other two.

Nancy, Oct. 20, 1864.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On a new saccharimeter.*—WILD has described a new saccharimeter which appears to possess great advantages over the well-known instrument of Soleil. In Wild's apparatus the incident light is polarized by means of a Foucault's calc-spar prism, which is capable of being turned round its axis, the amount of rotation being measured by means of a vernier applied to a small circle graduated to single degrees. The analyzer consists of a modification of Savart's polariscope, that is to say, of two crossed plates of quartz, each 20mm. in thickness, cut at an angle of 45° with the optical axis. The fringes produced are viewed with a small astronomical telescope of low power (objective 33mm. focal length, ocular 24mm. focal length). The telescope is adjusted for parallel rays, has cross wires, and a Nichol's prism in front of the eye piece. The tube containing the solution of sugar is of course placed between the polarizer and analyzer. The author instituted comparative experiments to test the degree of accuracy attainable with Soleil's instrument and with his own, employing for the purpose a solution containing 298 grams of cane sugar to the litre. The mean error of a single compensation in Soleil's instrument amounted to 1°, when the eye was properly protected, while it reached 1½° by direct observation in an ordinary room without exclusion of side light. From this it results that in the most favorable case there is with Soleil's instrument an uncertainty of ±13·9 gr. in a litre of the solution, or rather more than one per cent. With Wild's instrument the error of observation with white light amounted to 12′, which corresponds to an error of ±2·78 gr. of sugar in a litre of solution, and consequently to a degree of accuracy five times greater than with Soleil's apparatus. When this instrument was directed toward an Argand gas burner in a dark chamber, and a dark red glass was placed between the eye and the ocular, the error of observation amounted to only 3′. Finally, when a homogeneous soda flame was employed, the error amounted to only 1′. In the first case, the error in the quantity of sugar would amount to ±0·7 grs.; in the second, to only ±0·2 gr. in a litre.[1]—*Pogg. Ann.*, cxxii, 626.

W. G.

[*Note.*—Wild's Saccharimeter, independently of its purely technical

[1] Wild's Saccharimeter can be procured of Hoffman, optician, 3 Rue de Buci, Paris.

uses, will form a most acceptable addition to physical cabinets. To the chemist it offers the most precise method of determining the rotatory polarizing powers of liquids at present known, provided that homogeneous light be employed, since the characteristic lithium, sodium and thallium lines afford perfectly definite degrees of refrangibility. In Broch's measurements of the rotatory polarization in quartz by means of the fixed lines in the spectrum, the mean error of a single measurement varied from about 6′ to about 30′, according to the refrangibility of the light. In a series of measurements undertaken to determine the degree of accuracy with which the change of tint could be determined in plates of quartz placed between two Nichols' prisms and examined with white light, I found, as might be expected, that the error of observation increased as the intensity of the incident light diminished, the minimum error of estimation being about 15′. Hence it appears probable that the degree of accuracy attainable in Wild's instrument might be still farther increased by using a condenser so constructed as to give an intensely brilliant beam of parallel and homogeneous rays.—W. G.]

2. *On the mechanical energy of chemical action.*—Schröder van der Kolk has communicated an interesting and suggestive memoir upon the application of the theory of mechanical energy to chemical processes, especially in connection with the relations of chemical affinity to heat. We shall endeavor to give some idea of the train of reasoning in this paper, referring however to the original for more ample illustration. The author in the first place cites the opinion expressed by H. Sainte-Clare Deville, in his paper on dissociation, that all chemical compounds may be resolved into their elements by a sufficiently high temperature. In some cases the separated atoms reunite at a lower temperature; in other cases they remain separate. This gives us from this point of view two classes of bodies, and the author connects the phenomenon with the facts observed by Favre and Silbermann that certain combinations are effected with evolution, others with absorption of heat. The relation between the two classes of phenomena appears from the following considerations. If we conceive of a body at the temperature of 0°, and then apply heat, the body at a definite temperature will have absorbed a definite quantity of heat. This heat is applied partly in raising the temperature, partly in molecular changes, and partly in external work. The entire quantity of heat continually increases with the temperature, and heat is always taken up both in raising the temperature and in changing the state of aggregation from the solid to the liquid, and from this to the gaseous condition. To find the quantity of heat in the body after the process, we must subtract the quantity which is consumed in external work. The quantity remaining is termed by Thomson the mechanical energy of the body. Starting as we have done from 0°, we obtain in this manner only the accumulation of energy in the body from 0° to the particular temperature under consideration, and not its absolute quantity. Every body has therefore in any definite condition a definite quantity of energy. If now we consider two substances, as, for example, oxygen and hydrogen, which combine with each other under the influence of the electric spark, we have in each, before combination, a definite quantity of energy. During the combination, heat is evolved, and if we now cool the vapor of water

which is formed to the temperature of the components before combination, the steam will contain a quantity of energy less than that of its components by the quantity which is given out in the combination. We here suppose the combination of H and O to take place without external work. When the vapor of water is decomposed at the same temperature, just as much energy must be added as became free during the combination. Two cases may then occur: either the body has more energy than its components, or else the contrary is true. In the first case heat is given out in the decomposition, in the second, it is absorbed. It follows that heat must become free when we heat a body of the first class to the point of decomposition, and the components will not unite on cooling, since the quantity of energy in them is not sufficient for the formation of the compound at the same temperature. This could only take place in two cases: either the body must take up heat from the surrounding medium, which, however, has at most an equal temperature, or else the compound must become suddenly colder. So far as known, no such cooling occurs. From the above we obtain the following law: Bodies which evolve heat in being decomposed by heat are not again formed in the subsequent cooling. This law may be tested by the results of the experiments of Favre and Silbermann. These physicists found that protoxyd of nitrogen, NO, peroxyd of hydrogen, HO_2, chlorous and chloric acids, ClO_3 and ClO_5, evolve heat during decomposition, while the elements separated do not recombine on cooling. The same is true for the chlorid, iodid and sulphid of nitrogen, which are suddenly decomposed on heating, with evolution of heat, and the components of which do not recombine. The above law is also true when dimorphous or polymorphous bodies change their physical condition. Thus, aragonite on heating passes into calc-spar with evolution of heat; the converse change does not take place on cooling. Fused sulphur crystals (System V) on cooling become crystallized in System IV with evolution of heat; the converse change does not take place. Various other illustrations are given, and the author generalizes the law already stated as follows: When bodies on heating pass into a different physical state, with evolution of heat, the previous state is not resumed on cooling. It is of course possible that in certain cases the purely chemical affinity between the molecules may be so powerful that combination will ensue, heat being abstracted from the surrounding medium. The converse proposition is as follows:

If heat be absorbed in decomposing a body by means of heat, the contrary action will take place on subsequent cooling.

This proposition cannot be demonstrated theoretically, and is therefore not to be regarded as proved. It is frequently found by experiment to be true: thus, carbonate of lime, according to Favre and Silbermann, absorbs heat when decomposed by heating, and has therefore less energy than its components together. CO_2 and CaO recombine on cooling. On the other hand, much heat is evolved in slaking lime, and CaO, HO has therefore much less energy than its components. On heating, the hydrate is decomposed, and is again formed on cooling. In the formation of CO_2 and HO heat is evolved, and the compounds have therefore less energy than the components. The elements only unite at a high temperature, and the decomposition takes place at a still higher temperature.

On cooling, the components ought to recombine, but in point of fact this does not happen, though the components have much more energy than the products. The explanation of this depends upon the action of the purely chemical affinities, which at lower temperatures are not sufficient for the formation of the compound. Two conditions are in general necessary for the formation of a compound. First, a degree of chemical force or affinity sufficient for the combination, and secondly, the necessary energy. A single cause is insufficient. The author here distinguishes between reversible and unreversible processes. When a body changes under the action of heat, and with evolution of heat, it passes into a new condition of less energy, and therefore cannot in itself pass back into its original state. This is the unreversible process, and occurs most frequently. When heat, on the contrary, is absorbed, the new body has more energy, and may therefore on cooling pass of itself back into its previous condition. This is illustrated by the latent heat of fusion and evaporation. Other consequences of this theory are these; the heat of combination remains the same whether the compound be formed suddenly or by degrees; the heat of combination of a compound body is in general less than that of its components taken singly. The following examples serve to illustrate still further the application of mechanical energy to the explanation of chemical changes. An electric spark causes combination in two ways. Certain bodies, H and O, H and Cl, CO and O, &c., combine suddenly in unlimited quantities by means of a single spark, and with evolution of heat. Others, like N and O, combine only gradually; no heat is evolved, and the combination ceases with the cessation of the spark. The formation of ozone is also a case in point. In the first case, the energy of the components is greater than that of the compound. Sufficient energy is present, but not sufficient affinity. The electric spark here increases the affinity; some atoms of H and O combine, and the heat produced causes a further combination, since heat within certain limits increases affinity. In the case of the combination of N and O to form nitric acid, the components have less energy than the compound. Then the spark furnishes the requisite energy, and each spark yields only a definite amount of work. The case of NH_3 forms an exception which for the present must remain unexplained. The author also applies the theory to the catalytic action—so called—exercised by platinum upon H and O, but not upon N and O. In conclusion, the author takes ground against the assumption often made that the heat of combination is to be taken as the measure of chemical affinity. He considers that this heat, regarded as difference in energy, is the measure of the stability of the compound. Thus, water with the heat of combination expressed by 29413 has more stability than chlorhydric acid with a heat of combination of 23783 units. In each case, besides the requisite energy, the requisite affinity must be present. Thus, in a simple mixture of O and H no combination takes place until the affinities are sufficiently increased, although the necessary energy is always present. A further argument against considering the heat of combination as the measure of affinity is to be found in the fact that in certain combinations heat is absorbed, so that in these the chemical affinity would be negative.—*Pogg. Ann.*, cxxii, 439. W. G.

3. *On the replacement of hydrogen in ether by chlorine, ethyl and oxethyl.*—Lieben and Bauer have studied the action of monochlorinated and bichlorinated ether upon zinc-ethyl and ethylate of sodium, and have obtained results of much theoretical interest. By the action of chlorine upon ether, at ordinary temperatures, the author obtained a mono-chlorinated ether with the formula, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4Cl\end{matrix}\right\}O_2$. The action of zinc-ethyl upon this compound gives rise to two products which have respectively the formulas, $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.Cl\end{matrix}\right\}O_2$, and $\left.\begin{matrix}C_4H_4.C_4H_5\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, and in which, as will be seen, one or two atoms of hydrogen in the radical ethyl are replaced by one or two atoms of ethyl itself. When mono-chlorinated ether is heated with an alcoholic solution of potash, or with ethylate of sodium, an oily liquid is formed, which boils at 157° C., is heavier than water, and has a most refreshing and agreeable odor. The formula of this body is, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$, so that it must be regarded as ether in which one atom of hydrogen in the ethyl is replaced by chlorine and one atom by the radical, $C_4H_5O_2$, which the authors term oxethyl. By the continued action of ethylate of sodium upon the last mentioned substituted ether, the authors obtained an oil lighter than water, boiling at 168° C., and having the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5O_2\end{matrix}\right\}O_2$. In this, two atoms of hydrogen are replaced by two of oxethyl. Finally, the action of ethylate of sodium upon this body, $\left.\begin{matrix}C_4H_4Cl\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$, gives rise to a new liquid, boiling at 148° C. and presenting the formula, $\left.\begin{matrix}C_4H_4.C_4H_5O_2\\C_4H_4.C_4H_5\end{matrix}\right\}O_2$. The new substances described are evidently the types of a large class of similar substituted bodies, and deserve in themselves an attentive study, especially with reference to physical properties and to products of decomposition.—*Comptes Rendus*, lix, 445. W. G.

4. *On a new mode of preparing oxygen.*—Robbins has given a method of preparing oxygen which is particularly interesting from a theoretical point of view, and which may hereafter be of much practical value. The process is based upon an experiment due to Schönbein, and consists in pouring dilute sulphuric acid upon a dry and powdered mixture of three equivalents of peroxyd of barium and one equivalent of bichromate of potash. Ozone and antozone are given off simultaneously, and unite to form common oxygen, which escapes with effervescence.—*Cosmos*, xxvi, 386. W. G.

II. GEOLOGY.

1. *On buried stems and branches in Illinois;* by J. S. Bliss. (From a communication to the Editors, dated Columbus, Wisconsin, Nov. 7.)—While passing through the county of Adams, in the State of Illinois, four years since, I learned of the existence of a well, which had been dug the year before, from which small branches of trees and twigs had been obtained. I repaired to the spot, and found the well to be twenty-five feet in depth; and was informed that, at a depth of twenty-three feet, the owner

came upon a layer of *fine black soil*, full two feet in thickness, in which were found branches an inch in diameter; I procured a shovel, and obtained several specimens three-eights of an inch through, in a tolerable state of preservation. Just above this bed of *soil* the material was clay.

In another well, three miles from this, the same kind of soil was met with, after passing through clay for twenty-seven feet from the surface. In the soil twigs were found; but on being brought to the air they crumbled to pieces, though the *bark* and *heart* were plainly to be seen.

Four miles from the first mentioned well, there was another, still more remarkable, which was 30 feet deep. At a depth of 27 feet, the diggers came square on a log, twelve inches in diameter; they cut it off on both sides of the well, and on taking it out pronounced it to be *Black walnut;* but I think that it was probably some other kind of timber, that had been dyed in consequence of having been submerged, and that this caused the mistake. In the same soil was also found a *Buckthorn* in a tolerable state of preservation. The top of the tree was lying southeast.

Another fact, I have noticed in Wisconsin, is that stones protruding through the surface, such as those known as *hard-heads*, present their perpendicular sides to the north, northeast, and northwest. This is not confined to any small portion of the State, but to quite an extended region.

2. *On the Geology of Eastern New York;* by Professor James Hall and Sir William E. Logan. (Read before the Natural History Society of Montreal, Oct. 24, 1864, by T. Sterry Hunt.)—Professor James Hall and Sir William Logan spent a few days together last summer in examining some points of the geology of Eastern New York, and propose to continue their examinations next season, when we may expect from them a detailed account of their results. Their principal object was to compare the rocks of that region with some of those of Eastern Canada; and I have now permission, in the absence of these gentlemen, to lay before this Society some of the results of this exploration.

The shales of the Hudson River group, which are seen for a considerable distance north and south of Albany, disappear a few miles east of the Hudson, and are succeeded by harder and coarser shales, sometimes red or green in color, and passing into green argillaceous sandstones. These various strata, which are associated with concretionary and shaly limestones, are now recognized as belonging to the Quebec group. The line of contact between this and the much more recent Hudson River group has nowhere been clearly seen in this region, but the two series are readily distinguished by their differences in color, texture, and hardness,—differences which were formerly supposed to depend upon the partial metamorphism of the eastern portion, when this was looked upon as a part of the Hudson River group. The green sandstones and conglomerates of Grafton Mountain, formerly looked upon as a portion of the Shawangunk conglomerate, are recognized as belonging to an outlying portion of the Sillery formation. This mountain Professor Hall had found in a previous exploration (1844–45) to have, at a point farther south, a synclinal structure, and it probably lies in three low synclinal axes. The Sillery formation scarcely extends south of Rensselaer County.

Canaan Mountain is also apparently synclinal, and, while limestones appear in the valleys on each side of it, consists chiefly of slates, the highest beds being a hard green sandstone, sometimes shaly, without any of the conglomerates of the Sillery, although boulders and angular fragments of these are found in the adjacent valleys. To the east of this, Richmond Mountain, in Massachusetts, presents in its upper portion a compact green slate, passing upward into a harder rock similar to that of the summit of Canaan Mountain. To the southward, as far as Hillsdale, the sparry limestones of the Quebec group appear in the valleys, while the hills are of slate. Proceeding thence westward toward the river, only the lower portions of the Quebec group are met with, until we come upon the rocks of the Hudson River group.

Washington Mountain is also of slate, flanked by limestone, all of the Quebec group, and is probably synclinal in structure. The valley to the south of the mountain exhibits limestones, apparently alternating with slates. Columbia and Duchess counties appear to be mainly occupied by the shales of the Quebec group, with broad exposures of its limestones, until we approach the river to the westward, when the shales of the Hudson River group are met with, extending a considerable distance below the city of Hudson. The gneiss of the Highlands occupies the southeast part of Duchess County.

From Fishkill the explorers proceeded to Coldspring, crossing what Mather called the Mattewan granite, but which they found to be an altered sandstone. Soon after this they came upon the great gneiss formation of the Highlands of the Hudson, which continues beyond Peekskill. They failed to find the sandstone described by Mather as coming out at this place; nor was anything representing the Potsdam sandstone detected in approaching the Highlands from Fishkill, nor elsewhere along their northern limits. Near to Peekskill, in the valley of the creek, was found a low ridge of black slate, supposed to belong to the Quebec group, and a similar slate was observed along the north side of the Highland range, not far from the gneiss. The gneiss of the Highlands presents all the aspects and characteristics of that of the Laurentian system, as seen in northern New York and in Canada.

Further examinations are necessary to determine the extension to the northeast of the Laurentian rocks of the Highlands, and also the succession of strata to the southeast of them. The recognition of the Sillery and of the Quebec group in this region are great and important facts of its geology, and not less so the identification with the Laurentian system of the gneissic district of the Highlands, to which the interesting mineral region of Orange county and the adjacent parts of New Jersey doubtless belongs. This conclusion, although opposed to the views of Mather and Rogers, who looked upon the crystalline rocks of the latter region as altered Lower Silurian strata, is in accordance with the older observations of Vanuxem and Keating, and with the more recent ones of Professor Cook, according to all of whom the gneiss and crystalline limestones of Orange County and of New Jersey underlie uncomformably the Lower Silurian strata.[1]—*Can. Nat. and Geol.*

[1] See Am. Jour. Sci., [2], xxxii, 208, and also Kitchell's 2nd Annual report on the Geology of New Jersey, (1855,) page 134 and onward.

3. *Action of Glaciers;* by J. RUSKIN.—What a river carries fast at the bottom of it, a glacier carries slowly at the top of it. This is the main distinction between their agencies. A piece of rock which, falling into a strong torrent, would be perhaps swept down half a mile in twenty minutes, delivering blows on the rocks at the bottom audible like distant heavy cannon,[1] and at last dashed into fragments, which in a little while will be rounded pebbles (having done rough damage to everything it has touched in its course)—this same rock, I say, falling on a glacier, lies on the top of it, and is thereon carried down, if at fullest speed, at the rate of three yards in a week, doing, usually, damage to nothing at all. That is the primal difference between the work of water and ice; these further differences, however, follow from this first one.

Though a glacier never rolls its moraine into pebbles, as a torrent does its shingle, it torments and teases the said moraine very sufficiently, and without intermission. It is always moving it on, and melting from under it, and one stone is always toppling, or tilting, or sliding over another, and one company of stones crashing over another, with staggering shift of heap behind. Now, leaving out of all account the pulverulent effect of original precipitation to glacier level from two or three thousand feet above, let the reader imagine a mass of sharp granite road-metal and paving-stones, mixed up with boulders of any size he can think of, and with wreck of softer rocks, (micacous schists in quantities, usually,) the whole—say half a quarter of a mile wide, and of variable thickness, from mere skin-deep mock-moraine on mounds of unsuspected ice—treacherous, shadow-begotten—to a railroad-embankment, *passenger*-embankment—one eternal collapse of unconditional ruin, rotten to its heart with frost and thaw, (in regions on the edge of each,) and withering sun, and waste of oozing ice; fancy all this heaved and shovelled, slowly, by a gang of a thousand Irish laborers, twenty miles down hill. You will conjecture there may be some dust developed on the way?—some at the hill-bottom? Yet thus you will have but a dim idea of the daily and final results of the movement of glacier moraines;—beautiful result in granite and slate dust, delivered by the torrent at last in banks of black and white slime, recovering itself, far away, into fruitful fields, and level floor for human life.

Now all this is utterly independent of any action whatsoever by the ice on its sustaining rocks. It *has* an action on these, indeed; but of this limited nature, as compared with that of water. A stone at the bottom of a stream, or deep sea-current, necessarily and always presses on the bottom with the weight of the column of water above it—plus the excess of its own weight above that of a bulk of water equal to its own; but a stone under a glacier may be hitched or suspended in the ice itself for long spaces, not touching bottom at all. When dropped at last, the weight of the ice may not come upon it for years, for that weight is only carried on certain spaces of the rock bed; and in those very spaces the utmost a stone can do is to press on the bottom with a force necessary to drive the given stone into ice of a given density (usually porous); and, with this maximum pressure, to move at the maximum rate of about a third of an inch in a quarter of a hour! Try to saw a piece of marble

[1] Even in lower Apennine. "Dat sonitum saxis, et torto vertice torrens."

through (with edge of iron, not of soppy ice, for saw, and with sharp flint sand for feldspar slime,) and move your saw at the rate of an inch in three-quarters of an hour, and see what lively and progressive work you will make of it!

I say "piece of marble;" but your permanent glacier-bottom is rarely so soft—for a glacier, though it acts slowly by friction, can act vigorously by dead weight on a soft rock, and (with fall previously provided for it) can clear masses of that out of its way, to some purpose. There is a notable instance of this in the rock of which your correspondent speaks, under the Glacier des Bois. * * * *

[Mr. Ruskin continues with further explanation of his views, in the course of which he expresses the opinion that "the Glacier des Bois has not done more against some of the granite surfaces beneath it, for these four thousand years, than the drifts of desert sand have done on Mt. Sinai;" and "it never digs a hole" like the bed of a lake. He closes with a paragraph containing the following statement as to his early tastes and studies.]

I find it difficult to stop, for your correspondent, little as he thinks it, has put me on my own ground. I was *forced* to write upon Art by an accident (the public abuse of Turner) when I was two-and-twenty; but I had written a "Mineralogical Dictionary" as far as C, and invented a shorthand symbolism for crystalline forms, before I was fourteen: and have been at stony work ever since, as I could find time, silently, not caring to speak much till the chemists had given me more help.—*Reader, Nov.* 26, 1864, *the communication dated Denmark Hill, Nov.* 21.

4. *Geological Survey of California;* J. D. Whitney, State Geologist. —Paleontology, Volume I: *Carboniferous and Jurassic Fossils*, by F. B. Meek; *Triassic and Cretaceous Fossils*, by W. M. Gabb. 244 pp. Small 4to, 1864. Published by order of the Legislature of California.—The principal results of the geological survey of California, up to the present time, have been briefly stated in our last volume at p. 256. Their number and importance are sufficient to show that the survey could not have been in better hands. Through the labors of Professor Whitney and his assistants, we are beginning to have some definite knowledge of the geology of the western border of the continent,—of the distribution of its rock-formations, of the age and structure of its mountains, of the origin of its metallic veins, of the action of rivers and glaciers in educing the features of the surface, besides many other points of geological and economical interest. The Report on the general Geology, as far as the subject is completed in the yet unfinished survey, will, we understand, be soon published. The volume just issued is the first of the two on the Paleontology of California. It treats of the fossils of the Carboniferous formation, the oldest fossiliferous beds yet observed, and of the Triassic, Jurassic and Cretaceous formations, leaving the Tertiary for the second volume.

The Carboniferous fossils make but a meagre list, the rocks of the age having small extent. Twelve or fourteen species are all that are described. Three of them are referred by Mr. Meek, though with some hesitation, to the widely spread *Lithostrotion mammillare, Productus semireticulatus* and *Spirifer lineatus.*

The Triassic species, described by Mr. Gabb, are 28 in number; the Jurassic, described by Mr. Meek, 12; the Cretaceous, described by Mr. Gabb, nearly 300, the larger part being new, and all pertaining, according to Mr. Gabb, to the Upper or White Chalk. Some of the general facts respecting the extent and character of the formations have already been published by us in the article in our last volume above referred to.

A volume of plates will soon be ready for delivery. It will contain figures of all the new species. The plates are lithographic, and thirty-two in number; and, as we know from an examination of many of them, are excellent in drawing and engraving.

The work is brought out in the first style of the art, both as to paper and printing. We know nothing superior, in these respects, from any American press.

5. *Oil region of Pennsylvania.*—[Extracts from a communication by IRA SAYLES, dated Meadville, Crawford Co., Pa., Nov. 13, 1864.]—As shown in the Geological Map of Pennsylvania by Professor Rogers, the surface-rocks of the higher country in the oil region of western Pennsylvania are mostly Carboniferous, while those of the valleys appear to be Devonian of the Chemung and Portage groups. In the borings, a whitish sandstone, called the *first sand-rock* is met with at a depth of 70 to 200 feet; and wells penetrating only through this bed generally yield a thick oil. 100 to 200 feet below this, lies the *second sand-rock*, of similar aspect to the first; and from beneath this there is usually another "show of oil," a heavy oil, though less so than the former. From this rock proceed nearly all the wells of the Alleghany river, while the wells of French Creek, with few exceptions, descend only to the *first* sand-rock. Below the *second*, 100 to 200 feet, there is a *third* sandrock, and from beneath this comes the pure, limpid, light oil. To this depth descend most of the "flowing" or fountain wells, four of which, when first opened, (the Empire well, one of them,) deluged the region around with 3000 barrels of oil per day. The wells of Oil Creek are mostly in this rock.

The valleys have been selected in nearly all cases for the borings under the impression that breaks in the hills indicate chasms in the rocks beneath. But there appears to be no reason why oil should not be reached also by borings through the higher land. There are evidently two kinds of sources; open cavities affording the free flowing wells, sometimes intermittent; and the oil-bearing shales giving a slower yield, as if by a gradual percolation from rocks that partially retain it; and the latter at least should be encountered as well through the hills as the valleys, if the boring is carried down to the same level. Moreover, there is no certainty that fissures or chasms in the rocks may not underlie the hills.

It is a little remarkable that the oil from beneath the upper sandstones should be the heavier, while that from beneath the lowest is the lightest. Although there is no evidence of any connection between the oil repositories at these three different levels, the sources of the same level often communicate. This has been abundantly proved, by a sudden diminution in the supply of an older well in consequence of sinking a new one near by.

The quantity of oil produced now does not materially differ from that of two years ago—viz: nearly 6000 barrels daily. The number of wells

has been increased in that time; but about as many old ones have been abandoned as new ones opened. Fewer of the new wells are great *spouting* wells than formerly. Pumping wells are very constant in their yield.

6. *Petroleum in California.*—A petroleum region on the coast of Santa Barbara, near Buenaventura, in Southern California, has recently been visited by Prof. Silliman, Jr., from whose Report we extract a few facts. Ten miles North of Buenaventura there is a mountain ridge, 2000 feet in height, extending for 13 miles from east to west, which consists in part of bituminous shales supposed to be of either the Cretaceous or Tertiary period. The dip of the layer is 70° to 80° to the north. From these shales, mineral oil comes out in many places, and at some points very abundantly. One of the wells is 30 feet in diameter, and is full of a tar-like oil, boiling with the escape of marsh-gas. There is also an area of asphaltum three-fourths of a mile long by half a mile wide, exuding tar and rock-oil at numerous points, besides several other oil springs, the places of discharge in all exceeding twenty. The range of bituminous shales extends to the south, occurring at intervals for a hundred and fifty miles, and also to the north as far as Glenroy, in Santa Clara County, or about 80 miles from San Francisco.—*Extract from Prof. Silliman's Report.*

III. BOTANY AND ZOOLOGY.

1. *Dioico-dimorphism in the Primrose Family.*—Mr. John Scott, late of the Edinburgh Botanic Garden, has communicated to the Linnæan Society an elaborate paper, entitled, *Observations on the Functions and Structure of the Reproductive Organs in the Primulaceæ*, which has recently been published in the 8th volume of that Society's Journal of Proceedings. Mr. Scott has followed up Mr. Darwin's well-known researches by many additional experiments, has confirmed his results, and brought out others of no small interest. The paper is long, and, on the whole, clear and satisfactory, but with occasional vagueness or obscurity of language, from the attempt of an acute, but perhaps rather untrained mind to indicate the bearings of the subject further than they have been clearly made out. A good summary appended to the paper sets forth the principal points.

Of 54 species or forms of Primula which he has examined (many of them only in the herbarium),—

"36 are truly dimorphic, presenting both long- and short-styled forms; 13 in which the long- or short-styled forms, respectively, have alone been observed by me; and 5 species and one variety with non-dimorphic characteristics, i. e., presenting stamens and pistils of an equal length.

"The allied genera *Hottonia* and *Aretia* have also truly dimorphic species; whereas other allied genera, *Dodecatheon, Soldanella*, and *Cortusa*, are very generally characterized by species presenting the structural characteristics of the long-styled form only, without, however, any decreased fertility arising from their hermaphrodite conjunctions.

"The general differences of the two sexual forms may be thus briefly summed up:—First, the long-styled forms have pistils equalling in length

the tube of the corolla; stigmas usually larger and rougher; stamens attached to, or frequently below, the middle of the corolla-tube, whose diameter is thus expanded upward; pollen-grains generally smaller and more transparent. Secondly, in the short-styled form the pistil is short, not rising above half way up the corolla-tube; stigma generally smoother and depressed on the summit; stamens attached to the mouth of the corolla-tube, causing an abrupt expansion; pollen-grains generally larger and more opaque. According to all the trials, these structural differences are accompanied by equally remarkable functional differences,—the pollen of the long stamens being alone adapted to fertilize the long pistils, and the pollen of the short stamens to fertilize the short pistils. By applying, on the other hand, either form pollen to own form stigma, i. e. effecting a homomorphic union, the degree of fertility relatively to the above, or heteromorphic union, is greatly decreased. Analogous, though less striking, functional differences, however, occur without any appreciable change of structure, as shown by the *P. verticillata*, e. g., yielding a much higher grade of fertility by its diœcious than its hermaphrodite conjunctions. Such an instance from a genus whose members are so generally characterized by a sexual dimorphism, naturally leads me to regard it as indicative of the acquirement of similar characteristics. An objection to this view may be urged from the occurrence of species which, having no immediate affinity with any structurally dimorphic species, nevertheless present individuals incapable of fertilization by own pollen, though perfectly susceptible to reciprocal fertilization, either with another individual of the same species, or one of a distinct species. To this category, at least, those who disbelieve in the genetic affinities of organic beings will no doubt refer the case of *P. verticillata*, and simply regard it as further illustrative of our ignorance of the conditions upon which sterility, in its varied grades, depends. Those, on the other hand, who believe in the existence of these genetic relations will look with an intelligent interest upon these functional peculiarities of the *P. verticillata*, and regard them, mayhap, as the primary indications of a tendency to assume those remarkable sexual characteristics of the correlated species, and thus presenting an illustration of incipient dimorphism.

"The usual differences in the fertility of the heteromorphic and homomorphic unions will be best appreciated by giving the mean results from the unions of several species. Thus, taking the five heteromorphic and homomorphic unions given above, namely, *P. Auricula*, *Sikkimensis*, *cortusoides*, *involucrata*, and *farinosa*, we see, from the mean results of their combined products, that for every 100 seeds yielded by the heteromorphic unions, only twenty-four are yielded by the homomorphic unions —the heteromorphic thus exceeding the homomorphic unions in about the proportion of five to three! I have also shown the remarkable fact that the pollen of a distinct species will produce a much higher grade of fertility than an ordinary homomorphic union, i. e., a flower's own pollen!

"It is well known that A will fertilize B, and B will not fertilize A. I have given instances of this law with Primulas. I have also shown the new and remarkable fact, that of the two forms of the same species the pollen of the one, but not of the other, will fertilize a distinct species!

For example, the long-styled *P. Palinurii* can be fertilized readily by pollen of the long-styled *P. Auricula;* yet, after numerous trials, I have failed to effect a single union between the long-styled form of the *P. Palinurii* and the short-styled *P. Auricula.* How utterly inconsistent, then, are such facts with the teachings of those who would have us believe that an absolute causal relation exists between the sterility from hybridism and systematic affinity! On the other hand, how unequivocally do these cases show us that the greater or less facility of one species to unite with another is, as Mr. Darwin has sagaciously argued, 'incidental on inappreciable differences in their reproductive systems. And that there is no more reason to think that species have been specially endowed with various degrees of sterility to prevent them crossing and blending in nature, than to think that trees have been specially endowed with various and somewhat analogous degrees of difficulty in being grafted together in order to prevent them becoming inarched in our forests.'[1]

"Probably the most remarkable result from my observations is that when the dimorphic species cease to be dimorphic, their reproductive functions are greatly modified. Thus, in the case of the Cowslip, for example, we have seen that an ordinary homomorphic union yields about fourteen seeds per capsule, the heteromorphic about twenty-four seeds per capsule, whereas the form with stamens and pistils of an equal length yields, when fertilized with its own pollen, thirty-four seeds per capsule! Thus the non-dimorphic form by own pollen exceeds, first, the homomorphic unions in the proportion of 5 to 2, and secondly, the heteromorphic in the proportion of 3 to 2! Again, from the four different unions of the long- and short-styled forms with the non-dimorphic form, the seed-results in each case fall considerably below an ordinary homomorphic union: thus the mean results of the unions of the non-dimorphic with long- and short-styled are six seeds per capsule, whereas the pure homomorphic unions of the latter give an average of thirteen seeds per capsule—that is, as two to one!

"Connected with these are the remarkable changes in the fertility of the [differently] colored varieties of the Primrose, the red variety yielding no seed when fertilized by pollen of either yellow or white varieties: the reciprocal crosses of these, i. e. the pollen of the red variety applied to the stigmas of the yellow and white, are also absolutely sterile! On the other hand, fertile unions may be effected by the reciprocal crossing of the yellow and white varieties, though in every case we have found that the average seed-result of such unions is considerably under that of the pure unions of these forms.

"Whether or not the ultimate tendency of dimorphism is a complete separation of the sexes, I think we have the clearest testimony that dimorphism has not always been a genealogical characteristic; and furthermore, that the two forms did not *per saltum* assume these structural and physiological characteristics. I here allude to the evidence afforded by the non-dimorphic Cowslip—namely, the resumption of perfect hermaphrodism, and the occasional production of intermediate stages between this and the normally dimorphic. These, taking us back in the

[1] Origin of Species, 3d ed., p 299.

genealogical line, show us an original non-dimorphic progenitor, and the graduated plan by which it gave rise to a dimorphically characterized race."

The most noteworthy points are:—1. That, in a genus which has many species with dimorphous flowers, there are some which present, so far as known, only one of the two forms (say the long-styled, which is perfectly fertile *per se*), as well as others with stamens and pistil of equal length. 2. That the Red Cowslip, a clear variety of the common one, has become non-dimorphic, and, with this change of structure, has become much more fertile of seed than the heteromorphic unions of the Common Cowslip. And the Auricula offers a similar case. But, on the other hand, a non-dimorphic variety of *Primula farinosa* is less fertile. These changes in the genetic system of a species seem very remarkable. 3. Still more remarkable is the result of fertilizing the Red Cowslip with its ancestor the common yellow form; this union, and also that of the Common Cowslip fertilized by the Red, being very sterile;—thus supplying that *desideratum* which has been called for as a test, "a physiological species produced through variation." 4. That the two forms of a dimorphous species hybridize with very different degrees of facility with distinct species. And that such a cross sometimes yields more seed than either homomorphic unions with own pollen. 5. Mr. Scott confirms (though it is not stated in his summary) Mr. Darwin's statement, that the long-styled forms artificially fertilized with their own pollen are about twice as fertile as the short-styled similarly fertilized with its own pollen; while, as is well known, when left untouched, the short-styled is most fertile, owing to the relative position of the anthers and stigmas. So that the advantage of position in the latter is counterbalanced by an increased differentiation of pollen and stigma with respect to their mutual action, Nature thus rendering the short-styled equally dependent with the long-styled on external agencies,—thus keeping it in existence, and more completely effecting the great object of dimorphism,—the intercrossing of individuals.

We may here append the remark that the Thymelæaceous genus *Leucosmia* is dimorphous, and some species of *Drymispermum* exhibit one if not both of the two forms. A. G.

2. *Observations upon Dimorphous Flowers;* by H. von Mohl; in *Bot. Zeitung*, Oct. 1863, translated in *Ann. Sci. Nat., Bot.*, April, 1864.—These observations are principally upon that case of dimorphism in which, besides the ordinary hermaphrodite (but often infertile) flowers, there are other and surely fertile ones, of simplified structure, apetalous, or rather cryptopetalous, and which, "their development being as it were arrested in the bud," and fertilized without opening, we had long ago termed "precociously fertilized." Some remarks were made upon them in this Journal, in November, 1862, (p. 419), stating that Nature, in this case, takes as much pains to secure self-fertilization as she does in the other case of dimorphism (represented by *Primula*, *Houstonia*, &c.,) to secure cross-fertilization. This is the conclusion which Mohl also reaches after a careful investigation of the common instances, viz: in *Specularia*, *Impatiens*, *Viola*, *Oxalis*, &c., clearly showing that such flowers, with a possible exception in violets, must needs be self-fertilized. The record of his

own observations is preceded by a good history of what was known upon the subject, from the beginning made by Dillenius down to the communication of Michalet in the *Bull. Soc. Bot. de France*, 1860. That the little tympanum which covers the minute anthers and stigmas of *Specularia perfoliata* until after fertilization, is a rudimentary corolla, however, was shown by Dr. Torrey, in a paper read before the Lyceum of Natural History, New York, in 1830, but not published.

One of the most interesting things to notice, and a curious confirmation of the intention to self-fertilize in the cases here considered, is the unusual paucity and the greater activity of the pollen in the anthers of the small and closed flowers. In those of *Oxalis Acetosella*, as Mohl informs us, the larger anthers contain only about two dozen, the smaller scarcely a dozen pollen-grains,—only a few times more numerous than the ovules they are to fertilize, while in the normal blossoms they are in countless excess,—the economy in number being in proportion to the sureness that they will do their work. Moreover, these pollen-grains never leave the anther, but, (as was observed by D. Müller and by Michalet, but misunderstood, and neatly shown by Mohl,) send out their tubes to seek the stigmas, adjacent, indeed, but often at a considerable relative distance, these tubes directing their course with the same precision, and in the same mysterious manner, apparently, that they ordinarily do when having reached the ovarian cavity they seek the orifice of the ovules. This is well seen in *Viola*, where, though it is barely possible that extraneous pollen might sometimes be introduced, yet, in view of the immediate contiguity of the stigma to the line of dehiscence of the two more fertile anthers, through which the pollen-tubes are emitted, close-fertilization here cannot be doubted. So Mohl concludes that, "considering that these little flowers are constantly fertile; that in some plants they are the only ones that are fertile, while in others they are far more plentiful than the large flowers, and that upon these the propagation of the plant mainly depends, it results that it is not a general law in hermaphrodite flowers that Nature should favor fecundation by the pollen of another flower in preference to its own pollen." Mohl proceeds to state that self-fertilization is equally secured in some non-dimorphic flowers, and instances *Fumariaceæ* as a case where this is "absolutely necessary, the transport of pollen from one flower to the stigma of another appearing to be absolutely impossible, on account of the special structure of the corolla, and of the intimate connection of the exterior [interior?] petals which enclose the anthers and stigma." We had thought the same, although the nectariferous sacs should have made us pause; but Mr. Darwin called our attention to the great facility with which the cap or united tips of the inner petals would be pushed off to either side by a bee visiting the blossom and seeking honey from its nectary. To this it sufficed, as we thought, to answer, that pollen was actually falling from the anthers upon the stigma they surrounded a full day before the tips of the outer petals opened, i. e., before there could be any access whatever from without. The reply to this seeming demonstration duly came in Mr. Darwin's capital discovery that, in *Linum* and other cases, the pollen may be less potent or even impotent upon its own stigma, while that from another flower is prepotent! Still these little precociously-fertilized flowers, of various and diverse fam-

ilies, stand as veritable exceptions to the universality of the law which Darwin has ably deduced.

What causes the pollen-grains to emit their tubes without contact with the stigma, and indeed at a considerable distance from it, is an utter mystery. Mohl remarks here an analogy with the case of *Asclepias*, and that in *Viola*, *Specularia*, &c., as Brown has stated for *Asclepias*, there is no liquid poured out by the stigma which might somehow place the pollen in relation with the stigma.

Finally, Mohl calls attention to an early remark of Linnæus, that, in the year 1753, a considerable number of plants raised in the Upsal garden, for which the Sweedish climate was too cool, flowered clandestinely, yet produced fruit. We may note that in the Cambridge Botanic Garden, *Oxybaphus nyctagineus* copiously fruits from the bud in cool and cloudy weather: so do one or two Pavonias for a considerable part of the season. And, several years ago, *Nyctaginia capitata* of Texas seeded abundantly all one summer without expanding a single flower, or even producing a full-grown flower bud. The next year it blossomed freely.

Mohl justly notes that these diminutive bud-fruiting flowers are often produced earlier in the season, as in *Specularia*, *Impatiens*, &c., but sometimes later than the normal blossoms, as in *Oxalis* and *Viola*. And he shows that the former conform (he looking upon such flowers as preponderantly female) to Knight's hypothesis, (drawn from cucumbers and melons,) that a high temperature favors the development of male flowers, while a lower temperature is more propitious to female blossoms, —a view which Thury took as the starting point of his interesting investigation upon the production of sexes in the animal kingdom. But Naudin has appended to the French translation of Mohl's paper, an important note, in which he declares that his sedulous observations upon *Cucurbitaceæ* for the past ten years are far from confirming Knight's theory; that in this family male and female blossoms appear independent of conditions of temperature, but intimately connected with peculiarities of temperament, which vary from species to species, from race to race, or even among individuals of the same race. And he notes especially (what we suppose applies generally) that the races of melons, squashes, and gourds which have long been cultivated in northern Europe are comparatively more precocious, and need much less heat for maturing their fruit, than those of the same species which have been recently brought from tropical regions. Also that the same *Cucurbitaceæ* arrive at adult age, i. e., to the age for flowering and fruiting, far sooner under the hot and cloudless sun of the south of France than under the cooler and obscurer skies of Paris, where they vegetate richly, but hardly blossom. Naudin remarks, also, that many monoicous *Cucurbitaceæ* tend to become dioicous, without assignable cause; and, as especially fatal to Knight's view, that *Coccinia diversifolia*, cultivated simultaneously at Paris and at Hyères, in a great number of individuals, has produced only male blossoms at Paris, but an abundance of both sexes (monoicous) at Hyères. A. G.

3. *Najas major, Ruppia maritima, &c., discovered at Salina, N. Y.* —The September number of Dr. Seemann's *Journal of Botany* contains a *Revision of the Genus Najas*, by Professor Braun of Berlin. To the section *Eunajas* three species are assigned, viz., *N. major*, All., *N. muri-*

cata, Del., of Egypt, and *N. latifolia*, Braun, from the lake of Valencia, Venezuela. The section *Caulinia* has *N. flexilis*, which is found here and there in the North of Europe, and several varieties in the West Indies, Venezuela, &c., *N. arguta*, H.B.K., in New Granada, *N. minor*, All., in Europe, Egypt, and India, *N. fasciculata*, Braun, in India, and *N. graminea*, Del., in India, Egypt, &c. *N. major*, to which our attention is at present directed, under several varieties, is widely distributed throughout the Old World, in slightly brackish or fresh water, but it was also detected by Chamisso in the Sandwich Islands. What is equally remarkable, Prof. Braun has seen specimens collected in Florida by Cabanis.

Shortly before Prof. Braun's article came to hand, *Najas major* was discovered in Onondaga Lake, first by Judge G. W. Clinton, on the northern border of the lake (between Salina and Liverpool), and soon after by Mr. John A. Paine, Jr., on its western side, growing luxuriantly and abundantly, and quite fruitful. The marshy borders of the lake, as is well known, are brackish, at least in the vicinity of the salt-works, and abounding in maritime and submaritime plants. To the species formerly known there, the above gentlemen have added *Chenopodium glaucum*, *Leptochloa fascicularis*, and we believe some others. But a more interesting discovery, of a water-plant not before met with away from the sea coast, was recently made by Mr. Paine, he having detected *Ruppia maritima* in Onondaga Lake, along with *Najas major*, equally in a fruiting state.

A. G.

4. *Spontaneous return of hybrid plants to their parental forms.*—We have been prevented from preparing, as we wished to do, abstracts of the two rival essays upon Hybridity in Plants, viz., that of Mr. Naudin, of the Jardin des Plantes, and of Dr. Godron, of Nancy. The latter was published in full in the *Annales des Sciences Naturelles*, 4th series, volume xix, and also a part of the former, the whole memoir of Naudin, with details of the experiments and figures, having been accepted for publication in the *Mémoires* of the French Academy. The prize for which the essays competed were awarded to Naudin. A brief account of the two essays appeared a year ago in the Natural History Review. As respects one, the principal question, to use the summary of the Review, "the general result is, that Dr. Godron says: simple hybrids are absolutely sterile. Mr. Naudin says: hybrids are fertile whenever their anthers contain well organized pollen. Of 30 to 40 hybrids experimented upon, three-fourths were found to produce seeds capable of germination."

Upon another point of equal interest, Naudin's testimony is explicit, he maintaining that hybrid plants, however constant at first, tend in subsequent generations to a separation of the two specific elements, which are, as he expresses it, rather intermixed than truly combined, so that they would at length resolve themselves into the two parental types, or, by failure on one side, return to the one on the other. Naudin returns to this subject in a note in the *Flore des Serres* for July, 1864, where he gives the result of his experiments upon our common sorts of thorn-apple, *Datura Stramonium* and *D. Tatula*. These have more commonly been taken for varieties of one species; but their specific distinction has been maintained, especially of late, by various arguments. According to Naudin

they are truly distinct species, which do not sensibly vary. One always exhibits green stems and pure white flowers; the other, dark-purple stems and violet-tinged flowers. These two thorn-apples Mr. Naudin crossed, in 1855, and obtained a hundred or more hybrids, both *Tatula-Stramonium* and *Stramonio-Tatula*, both just alike, and exactly intermediate between the two species in the coloration of the stems and flowers. They had, however, the peculiarity of a gigantic size, attaining at least twice the size of their parents, and a tendency to sterility, which was manifested in the failure of all the flower-buds which were produced at the first forkings of the stems. The later flower-buds opened, however, and were perfectly fertile, the pods being as large and as full of good seeds as those of either parent. In 1861, these seeds (of *Stramonio-Tatula*) were sown, and produced a second generation like the first. Seeds of this crop were sown in 1862, and twenty-two seedlings were preserved for experiment. Five of these developed as *D. Stramonium* in all its purity, i. e., had green stems, white flowers, only the ordinary height, and all the flowers fertile, ripening their pods in the first forks, three months earlier than did their immediate parents. Nine individuals returned as completely to *D. Tatula* as the five did to *D. Stramonium*. Two others seemed to be *D. Tatula*, and were equally reduced in size and fertile from the first forks, but they still showed in their paler coloring a trace of the other ancestor. The remaining six of the twenty-two showed somewhat more of it, both in color and in the tallness and lateness of fructification; expressed numerically, Naudin estimates that they were, say, nine-tenths *D. Tatula* and one-tenth part *D. Stramonium*. "Here, then," says Naudin, "is a hybrid completely intermediate between the two parent species, which, left to itself, fecundated by its own proper pollen, is spontaneously dissevered at the second generation, dividing its offspring between the two species. And it is to be remarked that the division is very unequal, *D. Tatula* taking the lion's share, totally or nearly reclaiming seventeen individuals out of the twenty-two. This unequal division is common in hybrids, and sometimes goes to the extreme, one of the parents wholly disappearing from the hybrid progeny, which thus passes over entirely to the other species."

As to crossing species and their hybrids, in view of obtaining diverse and particular varieties, Naudin shows, accordingly, that, to obtain the desired results, the hybridizer should take pains to cross those mongrels which tend toward one parent with those which tend to the other, thus opposing the tendency to return to the specific types, which would otherwise surely take place. By doing this for a few generations, the two hereditary tendencies become, as it were, thoroughly confused, and a multitude of new variations break out in all directions, from which the amateur may select what he likes for preservation and further experiment. This is according to a principle first expounded by that excellent investigator, the late Louis Vilmorin. A. G.

5. *Flora of the British West Indian Islands;* by A. H. R. Grisebach, M.D.; Professor of Botany in the University of Göttingen. London, Lovell Reeve & Co. 8vo, parts VI and VII, 1864.—These parts, extending from p. 507 to p. 789, complete this most valuable work, which, as was intended, closes with the Vascular Cryptogamia. A full index of the

species is appended, and a list of colonial names. The preface gives an account of the circumstances under which the work was undertaken, and of the materials which the author has so sedulously and promptly elaborated. West Indian botany was very difficult and confused: "Almost all the principal authors who have written on West Indian plants belong to the last century, and consequently to the Linnæan school, and a general synopsis of West Indian plants has never before been attempted, not even by Swartz, whose Flora contains descriptions of his new species only, with a few remarks on allied forms." Moreover, the British West Indies offer only the separate fragments of a larger flora; and Trinidad, as its geographical situation indicates, naturally belongs to the flora of Venezuela and Guiana. The northern Bahamas might be supposed to have a vegetation very like that of East Florida, from which they are separated by the Gulf Stream; but this seems not to be the case. "Jamaica, again, from its mountainous character and more distant position, most of the leeward islands, from being wooded volcanos, and the majority of the windward ones, with a dry climate and a low calcareous soil, form three divisions of this tropical archipelago, which show as many peculiarities. Thus the whole of the British West Indies, as comprised in this flora, may be divided into five natural sections, each with a distinct botanical character." Altogether, they amount to about 15,000 English square miles, or nearly twice the area of Wales. But yet Hayti alone is nearly twice, and Cuba nearly thrice, as large as all the British Islands together, and not only far richer in vegetation, but far less explored, the publications of Jacquin, Swartz, &c., having been almost confined to the British possessions; so that it was with old species mainly, that Dr. Grisebach had to deal, those which were "the foundation, indeed, of our scientific knowledge of the flora of tropical America." And these "have so often been misunderstood that their synonyms are far more numerous than their numbers." A general West Indian Flora being out of the present question, we learn with interest that Dr. Grisebach is preparing a special paper on the geographical range of the West Indian plants, including the capital island of Cuba, which Mr. Charles Wright has so industriously and successfully explored through its length and breadth, and is expecting still further to explore.

We may here add the remark that Mr. Wright's third distribution of phænogamous plants and ferns—the fruits of his labors for the last three years—has just been made, consisting, in the fullest sets, of about 1800 species or numbers, (including some which have been redistributed under old numbers,), and that the greater part of them have already been determined by Dr. Grisebach, the now recognized authority in West Indian botany. An equally rich field probably remains in Hayti, to tempt an adventurous explorer. The Bahamas, also, bare as they seem to be, "have proved very interesting," and are thought to promise still many novelties.

The series of Natural Orders adopted in this work being different from the ordinary ones, and especially from that of the other colonial floras, mainly owing to the intercalation by Dr. Griesbach, of the apetalous or reduced forms among the polypetalous orders—where many of them clearly belong,—the author has added a tabular arrangement more in accordance with the Candollean series, and with that employed in the

new *Genera Plantarum* of Bentham and Hooker, which will be the natural companion of these colonial floras. These volumes are all written in English; and it is interesting to see how nearly at home the Göttingen Professor makes himself in a language foreign to him. A. G.

6. *Florula Australiensis: a Description of the Plants of the Australian Territory*, by GEORGE BENTHAM, F.R.S., P.L.S., assisted by FERDINAND MULLER, M.D., F.R.S. & L.S., etc., vol. II, *Leguminosæ* to *Combretaceæ*. 1864. pp. 521, 8vo.—That the second volume of this important work has been brought out so soon after the first, may be attributed, not merely to the indomitable industry and force of the author, but also to the fact that four-fifths of its pages are devoted to the great order *Leguminosæ*, to which Mr. Bentham has previously devoted much attention. In fact, to him the reformation of the order from the state in which DeCandolle left it, is almost wholly due. He first indicated, we believe, the floral characters which distinguished the Cæsalpineous from the Papilionaceous suborder, settled the tribes of the latter upon their present basis, and, besides various special memoirs, revised the *Mimoseæ* several years ago in a complete monograph. The huge genus *Acacia* is most largely Australian, where, indeed, of all Phænogamous genera it is most numerous in species; and the Phyllodineous Acacias are almost exclusively Australian. The present flora describes 293 species of *Acacia*, of which 271 are of the Phyllodineous section. These are so neatly analyzed by a tabular key, of the sort which Mr. Bentham makes so much of,—that the crowd of specimens in the herbarium may be most readily determined and arranged. *Chrysolalanceæ* are here recombined with *Rosaceæ*. *Saxifrageæ* (we know not why the form *Saxifragaceæ* is refused, and *Saxifrageæ* therefore used as the name both of an order and of a tribe) are well made to follow immediately after *Rosaceæ;* then come *Crassulaceæ*, of course; and *Droseraceæ*, retained as a good order. The volume is smaller than its predecessor, closing with *Combretaceæ*, not to sever the *Myrtaceæ*, so largely represented in Australia. A. G.

7. *Upon a form of budding in some Insect Larves.*—In the last number of the *Zeitschrift für wissenschaftliche Zoologie* are two articles of interest,[1] upon a form of multiplication by budding occurring in some larves of dipterous insects, and also containing a description of a new genus and species of Cecidomyians founded on one of the insects in which it was observed. It seems that Nicolaus Wagner published, in the memoirs of the Kazan Institute for 1862, an account[2] of a larval multiplication by a sort of internal budding, and that C. E. v. Baer confirmed the discovery last year in the 6th vol. of the St. Petersburg Bulletin, supplementing it with the full history of the larve, of its development into a perfect insect, and of the deposit of a few large eggs thus fructified. In July of 1864, Dr. Meinert published in the Danish *Naturhistorisk Tidsskrift* an account

[1] Weitere Erläuterungen über die von Prof. Nic. Wagner beschriebene Insectenlarve, welche sich durch Sprossenbildung vermehrt, von Fr. Meinert. Aus dem Danischen mit Bemerkungen übersetzt von C. Th. v. Siebold.

Die ungeschlechtliche Vermehrung der Fliegenlarven von Prof. H. Alex. Pagenstecher. Zeits. f. Wiss. Zool. xiv: 4 Oct., 1864.

[2] This was noticed in the Zeitschrift f. Wiss. Zool., xiii, 513, but the writer of this has been so situated that it wholly escaped him, and he has not yet seen that notice.

of the same facts as observed by him, and also a description of the perfect insect as a new genus and species, with the name *Miastor metraloas*. The first of the two articles cited above, is a translation of a part of Dr. Meinert's paper, with some remarks by Dr. Siebold, the editor, which, he says, was just completed when he received the second article. This latter is a well illustrated and detailed account by Dr. Pagenstecher of a similar larval multiplication occurring in another species of dipterous insect.

It will be sufficient here, after calling attention to these articles, to give the principle fact confirmed in them. This is, that an insect larve has been seen to have egg-like bodies within the abdominal cavity; that these have been seen to develop into larves, which finally bursting out became free and resembled in every way the stock larve; that then, or perhaps after a second similar larve-brooding, the larves developed into a perfect dipterous insect which laid eggs and started anew the circle of species-life; and lastly, that this is seen in more than one species.

There are some points not altogether agreed upon by the observers above referred to. One is as to the place and the material at and from which these larves are formed in the larve-stock. Dr. Meinert describes them as formed from the granular fatty mass of the abdominal cavity, which he regards as a sort of remnant of the embryonic yolk division. Dr. Pagenstecher, on the contrary, says he saw none of these fat cellules in the way of developing into eggs, and thought these latter wholly different from any cells approaching their size, and sums up as follows: "In some unknown part of the body, cells are set free, which, without being specially fructified, assume the character of eggs. These develop into embryos that exist in a sort of pupa state within the mother-body, and develop at the expense of the parent structures. Finally they break forth, the larve-stock dying, and become independent themselves."—pp. 414–5. W. C. M.

IV. ASTRONOMY.

1. *Discovery of a new Planet, Terpsichore*, (81).—Mr. Tempel, at Marseilles, discovered on the 30th of September a planet equal in brightness to a star of the 10th magnitude. The following elements are given by Mr. Tietjen:

Elements of Terpsichore (81).

T	=	1864, Oct. 23·0, B. m. t.	i	=	8° 45′ 43″·0.
M	=	351° 31′ 52″·0,	φ	=	7 32 35 ·4.
π	=	23 40 12 ·1,	μ	=	765″·494.
☊	=	2 48 29 ·3,	log. a	=	0·444043.

2. *New Comet.*—Another Comet was discovered on the 9th of September by Mr. Donati, at Florence. It was very faint, and seen with difficulty. Mr. Celoria, of Milan, gives the following elements, computed from observations of Sept. 9th, 11th, and 13th:

T	=	1864, July, 27·8829 m. t. of Milan.
☊	=	175° 11′ 57″·4,
π	=	190 10 34 ·7,
i	=	44 56 53 ·6,
log q	=	9·787184, motion retrograde.

The comet, according to these elements, passed its perihelion before those heretofore known as Comet I, 1864, and Comet II, 1864.

3. *Numbers of stars in the northern hemisphere.*—In the last edition of his *Wunder des Himmels*, Prof. Littrow gives a summary of the numbers of stars that are in Argelander's charts of the northern hemisphere.

From N. declination,	0° to 20°,	110,987	stars.
" "	20° to 40°,	105,082	"
" "	40° to 90°,	108,131	"

Classified according to magnitude there are,

Mag.	No. of Stars.	Mag.	No. of Stars.
1–1·9	10	6–6·9	4,328
2–2·9	37	7–7·9	13,593
3–3·9	128	8–8·9	57,960
4–4·9	310	9–9·	237,544
5–5·9	1,016		

There are, besides these, 60 nebulæ and 64 variable stars.—*Heis' Wochenschrift, Sept. 28th.*

4. *On the age of the moon's surface;* by J. Nasmyth.—The views I entertain on the subject in question are these, namely, that, as a direct consequence of the small mass of the moon, and its comparatively large surface, it must have parted with its original cosmical heat with much greater rapidity than in the case of the earth, and consequently the moon must have assumed a final condition of surface-structure ages before the earth had ceased from its original molten condition. And as the moon in all reasonable probability never possessed an atmosphere or water envelope (it certainly has none such now), while the earth has both, the action of the earth's atmosphere, and especially that of its ocean when it existed in the first instance as a vast vapor envelope, ere the earth had cooled down so as to permit the ocean taking up its final position *as an ocean*, this mighty vapor envelope must have retarded the escape into space of the cosmical heat of the earth millions of ages after the moon had assumed its final condition as to temperature.

Therefore it is from such considerations I am led to the conclusion that the surface features and details of the moon present to us a sight of objects the antiquity of which is so vast as to be utterly beyond the power of language to express, and scarcely less so for the mind to conceive.

Such considerations appear to me to enhance so vastly the deep interest which ever attends the examination and contemplation of the moon's wonderful surface, that I would earnestly urge those who agree with the soundness of these views to bear them in mind the next time they have an opportunity to behold the marvellous details of the lunar surface, as I am fain to think that in doing so the interest of what is there revealed to them will be rendered vastly more impressive.—*Proceedings Manchester Lit. and Phil. Soc.*, Nov. 15, 1864.

5. *On the probable error of a Meridional Transit-observation, by the eye-and-ear, and Chronographic method;* by Edwin Dunkin, Esq.—At the close of an elaborate discussion of the observations made at the Royal Observatory at Greenwich, in 1853 and 1857, presented to the Royal Astronomical Society in May, 1864, Mr. Dunkin says:

In laying before the Society the preceding abstracts, which are the results of a great mass of computations, I consider that I can with some confidence offer the following conclusions, which are derived from this discussion:—

(1.) In "*eye-and-ear*" observations, the probable error of a Greenwich transit observed in 1853 over *one wire* is $\pm 0^s{\cdot}078$, while that of a *complete* transit over the seven wires is $\pm 0^s{\cdot}029$. In *chronographic* observations, the probable error of a Greenwich transit observed in 1857 over *one wire* is $\pm 0^s{\cdot}051$, and that of a *complete* transit over the nine wires is $\pm 0^s{\cdot}017$.

(2.) There does not appear to be any certain difference in the probable error of transits of stars between the first and sixth magnitudes.

(3.) In "*eye-and-ear*" transits, for stars whose N.P.D. is greater than 60°, it would seem that the probable error of a transit increases slightly as the N.P.D. decreases; while in the *chronographic* transits the corresponding changes are insignificant.

(4.) In "*eye-and-ear*" transits, the personal discordances are liable to a considerable variation between the different observers; in *chronographic* transits, the differences between the observers are comparatively small. The general steadiness of observing by the latter method is very remarkable, and shows the great advantage obtained by its adoption.

(5.) The probable error of a Greenwich result for Right Ascension in the year 1853, as determined from "*eye-and-ear*" transits, is $\pm 0^s{\cdot}048$; while the corresponding probable error resulting from *chronographic* transits, in the year 1857, is $\pm 0{\cdot}034$. By arranging the separate results into groups, according to the stars' magnitude, it is found that no certain difference can be distinguished in the accuracy of the observed transits, excepting only that there is a tendency in both methods towards an increase in the probable error when transits of stars of the first magnitude are observed. The amount of increase in this instance, however, is in reality small, and is probably to some extent accidental. H. A. N.

5. *Shooting Stars in November*, 1864.—Arrangements were made at various places in the United States to watch for the shooting stars on the mornings of Nov. 13th and Nov. 14th. The sky was entirely overcast in most places, and nearly so in all. Through openings in the clouds, which would last a few minutes, occasional stars were seen, but whether there were more than usual it is impossible to determine. There was certainly nothing like the great display of 1833.

In San Francisco the sky was overcast. It is possible that some parts of California were favored with clear skies, and that we may yet obtain observations from them. Mr. Quetelet writes that a similar clouded sky prevented observation in Brussels. H. A. N.

6. *Supplementary note to the article on the Nebular Hypothesis.*—The form of Mars, as determined formerly, did not seem to agree with what we might expect from the primitive fluidity of the planet. The attention of astronomers having been recently called to the ellipticity of Mars, it seems now to be improbable that it has so large an ellipticity as has been heretofore received. (*This Journal*, [2], xxxviii, 436.) We thus see that this unconformable case is likely to yield to the requirements of the nebular hypothesis. We may venture to add that it is extremely improbable that Mars has an ellipticity differing much from that of the earth. D. T.

VI. MISCELLANEOUS INTELLIGENCE AND BIBLIOGRAPHY.

1. *National Academy of Sciences.*—The Report of the National Academy of Sciences, made by the Academy to the General Government, for the year 1863, has been published. It forms an octavo volume of 118 pages, with several maps. It contains only such Reports as had been made by committees of the Academy on the call of the General Government, with a brief statement of the operations of the Academy during the year 1863. The more important contents are a Report on the protection of bottoms of iron vessels from corrosion, etc.;—Report of the chairman of the Compass committee appointed to make experiments for the correction of local attraction in vessels built partly or wholly of iron;—Report of the committee appointed to examine the "Wind and Current Charts" and "Sailing Directions" issued from the Naval Observatory.

At the last meeting of the National Academy, held in August at New Haven, Ct., three vacancies in its list of members were filled by the election of Dr. J. C. Dalton, of New York, Leo Lesquereux, of Columbus, Ohio, and S. F. Baird, Assistant Secretary of the Smithsonian Institution.

2. *American Philosophical Society.*—The *Magellanic premium* of a gold medal has been awarded by the Philosophical Society to Mr. Pliny Earle Chase, for the discovery of numerical relations between gravity and magnetism.

3. *Medals of the Royal Society.*—At the Anniversary meeting of the Royal Society, on Wednesday, the last day of November, the Copley medal was awarded to Charles Darwin, Esq., F.R.S., "for his important researches in Geology, Zoology, and Botanical Physiology;" and the Rumford medal to J. Tyndall, F.R.S., for his researches on the Absorption and Radiation of Heat by Gases and Vapors.

4. H. R. Schoolcraft.—This distinguished traveller, and investigator of the manners, history, and language of the American Indian tribes, died at Washington on the 19th of December, in the seventy-second year of his age.

5. F. W. Struve, the celebrated astronomer of Pulkowa, Russia, died on the 23d of November last. His son, Otto Struve, was appointed his successor last year.

6. *Thoughts on the Influence of Ether in the Solar System, its relation to the Zodiacal Light, Comets, the Seasons, and periodical Shooting stars.* By Alexander Wilcocks, M.D. Extracted from the Transactions of the American Philosophical Society.—Dr. Wilcocks advances the hypothesis that there is a constant circulation of ether induced by the heat of the sun; that the cold ether descends near the poles of the sun, and that heated ether ascends from the sun's equator, or rather from a parallel of solar latitude just north of the equator; that this heated current as it rises is compressed by the cold ether into a thin conical sheet; that the ether bears with it matter capable of reflecting light, and thus causes the zodiacal light; that the tails of the comets are lighter than the ether, and arise for that reason from the sun, except when near the poles of the sun, when they are made to descend in the vortices of descending ether, so as to present double and multiple tails; that twice in the year, in August and November, the earth plunges through the sheet of warm

ether, causing a warm period in each month, the dog days and the Indian summer; and that in the middle of these two periods it causes the return of the August and November meteors.

Dr. Wilcocks has brought together and framed into a plausible hypothesis many facts, some of them hitherto not connected with any theory. But we do not believe that astronomers and physicists will be likely to accept his explanation. What he says relative to the seasons, and to the shooting stars seems peculiarly open to criticism. Why, for instance, the two periods of heat (supposing them to have an existence,) should not, according to his hypothesis, be equi-distant from the 5th of June and 5th of December, the two days when the earth is in the sun's equator, he does not explain. For the middle day of one period he names the 11th of August, which is 67 days from the 5th of June, while that of the other, which he calls the 12th of November, is only 24 days from the 5th of December. In what way the periodical star-showers could possibly be produced by such an ether we find it difficult to imagine. H. A. N.

7. *Report of the Superintendent of the Coast Survey, showing the progress of the Survey during the year* 1862. xx and 434 pp., 4to, with 49 diagrams and charts. Washington, 1864.—Besides the General Report on the Survey and its maps, there is the usual Appendix of Special Reports relating to various collateral physical observations. The subjects include—Tide tables for mariners; Cotidal lines for the Gulf of Mexico; Longitude of America from Europe, by Prof. Peirce; Lunar tables by the same; Longitudes in Maine, Alabama and Florida, by B. A. Gould; various series of Magnetic observations; Solar spots; Bessel's periodic functions; calculation of the depth of the Pacific from the earthquake wave of Simoda; Origin of the Florida Reef, by E. B. Hunt, etc.

8. *Eulogy on Joseph S. Hubbard;* by B. A. Gould. 44 pp., 12mo. —This eulogy was written in compliance with the call of the National Academy of Sciences, which in its constitution provides for such a tribute in commemoration of each member that may be removed by death. Prof. Gould had in his friend a congenial and inspiring theme. High mathematical powers combined with astronomical tastes, an energetic, investigating mind, and a frank and genial disposition tempered by a true Christian spirit, had accomplished much for science. The tribute is admirable as a biographical notice, appreciative, eulogistic but not beyond justice, and of special value as a chapter in the history of American astronomy.

9. *Chambers's Encyclopedia.*—A new volume—the Sixth—of this popular Encyclopedia, a work well filled out and illustrated in the departments of science, as well as in other varied branches of knowledge, has just been issued by the American publishers, J. B. Lippincott & Co., of Philadelphia. The volume (826 pp.) carries the alphabet nearly through N.

10. *Abstracts of Meteorological Observations made at the Magnetical Observatory, Toronto, Canada West, during the years* 1854 *to* 1859, *inclusive.* 136 pp. 4to. Toronto, 1864. *Results of Meteorological Observations made at the Magnetical Observatory, Toronto, during the years* 1860, 1861 *and* 1862. 84 pp. 4to. Toronto, 1864.

11. *Review of American Birds;* by S. F. Baird.—The publication and issue of this work has already reached the 148th page.

12. *Monograph of the Bats of North America;* by H. Allen, M.D.—Smithsonian Miscellaneous Collections, No. 165. Washington. 86 pp., 8vo, with numerous wood-cuts.—The species included in this important monograph are those of North America north of Mexico.

13. *American Journal of Conchology.*—A prospectus has just been issued by G. W. Tryon, Jr., of Philadelphia (625 Market st.), proposing the publication of a Quarterly Journal of Conchology, and soliciting subscriptions. Price ten dollars a year; single numbers three dollars. We commend it to all interested in the progress of science.

The Plurality of the Human Race; by G. Pouchet. Translated from the French by H. J. C. Beavan. London, 1864. Published for the Anthropological Society.

The Phenomena of Hybridity in the genus Homo, by Dr. Paul Broca. Published for the Anthropological Society.

Reptiles of British India; by Albert C. L. Gunther. Published by the Ray Society. 1864.

A History of the Spiders of Great Britain and Ireland; by John Blackwall, F.L.S. Published by the Ray Society. 1864.

Metamorphoses of Man and the lower animals (in French); by A. de Quatrefages. Paris, 1864.

Beschreibung und Eintheilung der Meteoriten; von Gustav Rose. 162 pp., 4to, with 4 plates. Berlin, 1864.

Meteorologische Waarnemingen, of the Netherland Meteorological Institute, for 1863. 326 pp., 4to. Utrecht, 1864.

Proceedings of the American Academy of Arts and Sciences, Vol. VI.—Page 97, On the Aluminates of Baryta; *W. P. Dexter.*—p. 106, Embryology of Asteracanthion berylinus, &c., with a plate; *A. Agassiz* [noticed at p. 130, vol. xxxviii, of this Jour.]—p. 114, On the velocity of light and the Sun's distance; *Lovering.*—p. 143, On the observed motions of the Companion of Sirius; *T. H. Safford.*—p. 166, Analysis of a Dacotah Meteorite; *C. T. Jackson.*—p. 169, On the new form of the Achromatic Object-glass introduced by Steinheil, with a plate; *Bond.*—p. 177, List of new Nebulæ seen at the Observatory of Harvard College; *Bond.*—p. 182, On Streptanthus and the plants referred to that Genus; *A. Gray.*—p. 188, Revision and arrangement (mainly by the fruit) of the North American species of Astragalus and Oxytropis; *A. Gray.*—p. 241, On the causes of Insanity among women; *Storer.*—p. 242, On the R. A. of the pole star as determined from observation; *T. H. Safford.*—p. 251, On a process of organic elementary analysis by combustion in a stream of oxygen gas; *C. M. Warren* [see last vol. of this Jour., p. 387].—p. 263, Observationes Lichenologicæ; *E. Tuckerman.*—p. 288, Investigation on the best approximate representation of all the mutual ratios of k quantities by those of simple integers; *Oliver.*

Proceedings of the Acad. Nat. Sci. Philadelphia. No. 4, Sept. and Oct. 1864.—Page 181, Limits and relations of the Raniformes; *E. D. Cope.*—p. 183, Description of a Gar-pike supposed to be new; *A. Winchell.*—p. 186, New genera and species of N. A. Myriapoda; *H. C. Wood, Jr.*—p. 187, Note on the Paralepidoids and Microstomatoids, and on some peculiarities of Arctic Ichthyology; *T. Gill.*—p 189, Synopsis of the Cyclopteroids of eastern N. A.; *id.*—p. 194 and p. 214, Synopsis of the Pleuronectoids of California and Northwestern America; *id.*—p. 199, on the affinities of several doubtful fishes; *id.*—p. 208, Note on the family of Stichæoids; *id.*—p. 211, Notes on shells, with descriptions of new fossil species and genera; *T. A. Conrad.*—p. 224, On the characters of the higher groups of Reptilia squamata, and especially of the Diploglossa; *E. D. Cope.*—p. 231, On a blind Silurid, from Pennsylvania; *E. D. Cope.*—p. 234, Fasti Ornithologiæ; Obituary of P. L. S. Müller; *J. Cassin.*

Proceedings of the Essex Institute, Salem, Mass.—Vol. III, includes years 1860 to 1863. Vol. IV commences with the year 1864. No. 3 of Vol. IV, (July, August and September,) contains the conclusion of *J. A. Allen's* Catalogue of the Birds of Massachusetts, and a paper on the habits of some species of Humble Bees, by *F. W. Putnam.*

Journal of the Portland Society of Natural History, Vol. I, No. 1. Portland, Maine.—This number is occupied by E. S. Morse's paper on the Terrestrial Pulmonifera of Maine, noticed in the last volume of this Journal (p. 303).